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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

VOL. L.—FOURTH SERIES.
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Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—————

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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Page 457, line 9 from bottom, *for* 0 *read* O.
— — line 7 from bottom, *for* C Q *read* O Q.
— 466, lines 5 and 6, *for* intersection *read* intersections, and *dele* and
to the origin.
— — line 13, *for* flow-line *read* straight line.
— 471, first line of § 42, *for* § 39 *read* § 38.
Plate IX. fig. 3.—The middle point of the straight line A B should be
marked O.
Plate X. fig. 6.—The *circle* should not extend into the left-hand half
of the figure, or should be shown there only by a dotted
line.

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Page 281, line 12 should read :—

$$a' = B_2 \mu^2 + B_4 \mu^4 + \ldots + \&c.$$

Page 316, lines 6, 7 should stand :—

$$= \frac{400}{\dfrac{3}{4} \dfrac{P}{5}} k,$$

$$= 533 \text{ miles.}$$

And *dele* line 8.

I. *On the Temperature attainable by Rock-crushing and its Consequences.* By ROBERT MALLET, *F.R.S.*[*]

IN developing the theory of volcanic heat and energy embraced in his paper "On the Nature and Origin of Volcanic Heat and Energy" (Phil. Trans. Part I., 1873), the main object of the author was to prove that the annual work of secular contraction in our globe, when transformed into heat, was more than adequate for the supply of volcanic activity existing upon our planet. While indicating generally the circumstances which must attend as results of the descent of the exterior shell upon the more rapidly contracting nucleus, it was not necessary to his argument to follow into detail the mechanism of local dislocation and crushing due to such descent. Nor would the limits of his paper admit of his entering into much detail as to the circumstances attending subterranean dislocation and crushing of rocky matter, or pointing out how some of these must greatly tend to exalt the temperatures due to the transformation of the mechanical work locally done. It was necessary to a truthful examination of the question whether or not the annual supply of heat transformed from the work of secular contraction were sufficient to meet the demands of existing volcanic action, that he should not overrate the work so transformed; and accordingly, in determining by experiment a measure for the amount of that work, the author viewed the work of crushing of unconfined or unsupported masses alone as the source of heat, this method being that only which could afford perfectly trustworthy experimental results. He paid no regard to the additional work that must attend the collision and friction

[*] Communicated by the Author.

of already crushed masses in the further progress of their defor-
mation and forced transport to points more or less distant from
those at which the crushing had taken place.   The work of
crushing in free air was capable of rigid determination ; the
work of subsequent deformation and transportation can only
admit of estimation upon assumed data, and these necessarily of
a somewhat arbitrary character, seeing how little we know accu-
rately of the nature and disposition of the rocky materials of our
earth's crust, except at the most inconsiderable depth from its
surface.   Nor were any of the circumstances pointed out by
which high temperatures are capable of being attained locally
in rocky masses crushed beneath our surface and which we
must assume as those actually occurring in nature.   The
writer's object here is to point out, 1st, that, taking the annual
supply of heat from transformed work of contraction, by expe-
riment in the way he has done, the result, though more than
sufficient to sustain his theory, affords alone no complete mea-
sure of the highest temperature that may through its means be
locally developed ; 2ndly, to answer some doubts which have
been raised as to whether the temperature to which subter-
ranean rocky masses can become raised by the heat evolved in
their crushing and transportation of particles can be sufficient to
bring more or less of these at such foci of crushing and disloca-
tion to the fusing-point of such materials, which the author in
his original paper assumes to be 2000° Fahr.

Professor Hilgard, occupying the chair of geology in the Uni-
versity of Michigan, U. S., in an able paper published in the
American Journal of Science, vol. vii. June 1874*, has, in terms
as clear as they are courteous, pointed out these *lacunæ* in the
author's original paper in the following passage :—

    " One point, however, must strike every reader of the original
memoir, viz. the preeminence given by Mallet to the *crushing of
solid* rock as the means of producing heat and fusion.   One
would naturally look to the results of his experiments on this
subject for the proof of the efficiency of this agency.   But we
find that the maximum *temperature* resulting from the crushing
to powder of the hardest rock is something over 217° Fahr.
This, then, represents the maximum increment of temperature
that can be rendered efficient toward the fusing of rocks by the
crushing process under the most favourable circumstances, viz.
upon the supposition that it takes place instantaneously, or
under such circumstances that the heat cannot be conducted
away, and, further, that the resistance of the rock has not been
materially diminished by the downward increase of hypogeal
temperature.   At the most moderate depths at which volcanic phe-

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* Phil. Mag. July, 1874, p. 41.

nomena can be supposed to originate the last-mentioned factor must exert a very considerable influence, reducing materially the available heat-increment. Hence the numerical results of Mallet's laborious experiments on rock-crushing, however interesting and useful as affording a definite measure of the thermal effects producible by this means, yet fail to carry conviction as to the efficacy of this particular *modus operandi* in reducing large masses of solid rock to fusion, unless essentially supplemented by friction, not so much of rock walls against each other, but more probably by the heat produced within more or less comminuted *detrital* or *igneoplastic* masses by violent pressure and deformation.

" It may be doubtful what would be the physical and thermal effect of enormously great pressures upon rock powder such as was produced in Mallet's experiments ; but it would seem that if *made* to yield, the frictional effect must produce very high temperatures. *A fortiori,* solid detrital masses of variously sized fragments intermingled (such as, rather than powder, would be likely to result from steady pressure), yielding rapidly under great pressures, might, under the *combined influence of friction and rock-crushing,* well be supposed to reach the temperature of fusion, which a simple crushing of a solid mass by pressure would have failed to produce. Mallet mentions the probable influence of friction and of the squeezing of igneoplastic masses, but does not attach to these agencies such importance as they seem to me to deserve.

" Of the complex thermal effects of the movements of detrital masses under great pressure Mallet's figures of course offer no measure whatsoever ; nor is this, or even the thermal coefficients resulting from his rock-crushing experiments, at all necessary to the establishment of the postulates of his theory."

Subsequently the Rev. O. Fisher, in a paper read before the Geological Society of London, May 12, 1875, entitled " Remarks upon Mr. Mallet's Theory of Volcanic Energy," has repeated the observations of Professor Hilgard, and extended his objections to the author's theory in general in a way which appears not warranted. It will be sufficient here to quote the following from Mr. Fisher's paper :—" Indeed the form in which the objection to Mr. Mallet's reasoning suggested itself to my mind on first reading his paper was simply this. If crushing the rocks can induce fusion, then the cubes experimented upon ought to have been fused in the crushing; and I still adhere to this simple mode of expressing my objection." Again :—" He considers that the heat so developed may be localized, and that the heat developed by crushing, say 10 cubic miles of rock, may fuse 1 cubic mile. But I ask why so ? The work is equally distributed throughout ; why should not the heat be so also ? or if not, what determines

the localization ? For example, suppose a horizontal column 10 miles in length and 1 in sectional area to be crushed by pressure applied at its ends, which of the 10 cubic miles is to be the one fused ? But if no cause can assign one more than another, it is clear that they will all be heated by 170° and none of them fused."

If a cube of rock, which in free air is found to crush under a certain pressure, be imagined situated deep within a mass of similar rock and there crushed, it does not admit of dispute that the work necessary to effect crushing must be largely increased; the particles of the cube and of the entire mass of surrounding rock are under the insistent pressure of the superincumbent rock in a state of elastic equilibrium.   It follows, therefore, that the pressures of the surrounding rock produce the same effect upon the cube as regards resistance to crushing as if they were cohesive forces acting within the cube; and the work necessary to crush the cube by its finally giving way, in whatever direction this *encastrement* by pressure may be least, will be increased over that which would crush it in free air nearly in the ratio in which the imaginary cube is exposed to external pressure greater than that in air. Thus, if the cube of Guernsey granite (No. 12, Table I. Phil. Trans. part 1, 1873, p. 186) which required 4,336,712 lbs. per square foot to crush it in air, equivalent to a superincumbent column of the same rock of the mean specific gravity 2·858, or weighing 178·3392 lbs. per cubic foot, be supposed situated at a depth of ten to twenty statute miles, it will require rather more than 2·14, or, at twenty miles, 4·28 times as much pressure upon two opposite faces to crush it that it did when in air; and if we assume the displacement of the crushed particles after crushing to be the same as in the case of the cube crushed in air, then the work and the heat due to its transformation will be also 2·14, or 4·28 times as great.   And as in the case of the cube crushed in air the heat developed was sufficient to fuse (at 2000° Fahr.) 0·108 of its own volume, or, in other words, the crushing of 10 cubic feet of the rock would be required to raise to that point one cubic foot, then in the case of the imaginary cube situated at the depth of ten miles enough heat would be evolved by the work of crushing each cubic foot to fuse 0·231 cubic foot, or, at twenty miles, to fuse 0·462 cubic foot of the same rock, or nearly half the volume crushed,—and this assuming that the initial temperature of the rock at 10 or 20 miles depth was only 57° Fahr. as in the author's experiments, instead of from 500° to 1000° Fahr. or more as it may be at 10 to 20 miles depth. Therefore, under the pressure due to a depth of 20 miles and an initial temperature of 1000° Fahr., the heat developed by the work of crushing each cubic foot of rock will be sufficient to

fuse its own volume. Thus also if we assume the fusing-point of the rocks not to be 2000° Fahr., as indicated by the author's experiments on the cooling of slags, but considerably higher, say 2500° or more, we have still a sufficient supply of heat due to crushing alone to bring 0·8 of the entire volume to the fusing-point.

These considerations, apart from all others yet to be adverted to, appear fully sufficient to refute the Rev. O. Fisher's first objection above quoted; indeed the statement that if under any circumstances and in the rock-masses of nature " crushing can induce fusion, then the cubes experimented upon ought to have been fused in the crushing," seems as unsupportable as it would be to affirm that no heat is developed by the slow oxidation (*eremacausis*) into water and carbonic acid of a pound of wood, which when burned develops a well-known amount of heat.

The depths above assumed do not widely differ from those at which the foci of earthquakes have been found by the author (Report on Neapolitan Earthquake) in 1857, and by others since that time, and which may be presumed to indicate in some degree the possible depth of volcanic activity.

The writer now proceeds to reply to the second objection of the Rev. O. Fisher as above quoted, which appears to him based entirely on a misconception of the physical conditions involved. Let us consider what will happen in the case of a prism or column of rock crushed against the face of an unyielding mass. If the prismatic mass be not homogeneous throughout, crushing will commence at the weakest place; if it be perfectly homogeneous, crushing will commence and continue where the prism is in contact with a fixed mass, and that whether the prism be crushed at one or both ends—because it is at such surface of contact that the compression of the particles of the prism is greatest, and where therefore the elastic limit of their cohesion is first and successively overpassed. This may be seen illustrated in the stonework of buildings the material of which is overloaded, where crushing or spalling off of the ashlar stones only occurs at and near the joints *. In either case, whether the prism be homogeneous or not, the crushing must be localized either to the end or ends of the prism, or to the plane of weakness where it first yields, and which then becomes the crushing surfaces of two opposed prisms. It is these physical conditions which " determine the localization " of crushing in the prism, and which conditions have been disregarded in the Rev. O. Fisher's objection. Let us now consider the subsequent effects of the

* See also E. Hodgkinson's experiments on the directions of fracture of crushed materials, Brit. Assoc. Report, vol. vi. ; and Tredgold on Cast Iron, by Hodgkinson, part 2, p. 319, and plate 1.

successive crushing of a column or prismatic mass of rock, one extremity of which is continually urged against the face of a fixed mass of rock which does not yield, a case which approximates to that which most frequently occurs in nature, and which, to fix our ideas, we may suppose presents a face for crushing of one square foot; and being continually urged forward, and the pressure being greatest where the pressing column comes into contact with the fixed mass of rock, the extremity of the column supposed homogeneous, or the parts adjacent thereto, are continually crushed by a succession of *per saltum* movements. The first cubic foot of the column that is crushed has its temperature raised, let us suppose, by the minimum of 217°. The crushed fragments at this temperature are pushed aside by the advancing column, whose extremity is thus surrounded by crushed material at a temperature of 217°, and the second foot in length of the column becomes crushed. But the material of this second cubic foot is at a higher temperature before it is crushed than was the first cubic foot; so that the heat due to the transformed work of crushing of each successive cubic foot of rock raises its temperature to a higher point than that of the preceding one, because each successive cubic foot at the instant before crushing is at a temperature already higher than the preceding ones, resulting from the heat taken up by the uncrushed column from the hotter portions of material surrounding it that have already been heated by crushing; so that, if T be the temperature produced in the first cubic foot crushed, and t be the temperature of the crushed material which communicates a portion of its heat to the next cubic foot crushed, the temperatures of successive cubic feet crushed may be illustrated by some such series as the following:—

<div align="center">Cubic feet crushed.</div>

No. I.	No. II.	No. III.
T	$T + \dfrac{t}{n}$	$T + \dfrac{t}{n} + \dfrac{t}{m}$ ... &c.

We have here supposed the column crushed at atmospheric pressure; but if crushed under an insistent column of 20 miles, then the temperature T would be 4·28 times 217°=928°, and the subsequent temperatures correspondingly increased.

No limit arises to this continual augmentation of temperature while the rock retains its rigidity; after that has been seriously impaired or lost, any further exaltation of temperature apart from the detrusion or transport of fragmentary matter, as hereafter referred to, becomes dependent upon the deformation and detrusion of a more or less plastic mass. It is well ascertained, however, by observation on a great scale, that granite remains rigid at a

temperature nearly approaching the softening point of cast iron*; so that a large range of rigidity must exist for the exaltation of its temperature in the way above suggested; and in the state of aggregation which we are warranted in supposing rocky masses to exist at considerable depths, it is probable that this range of rigidity would be even further extended than in the case of granites found at or near the present surface of our globe.

There is a close analogy between the conditions of gradual exaltation of temperature above sketched, and those by which aërolites, flying at an immense velocity through our atmosphere, are heated from the temperature of the stellar spaces to that of incandescence or even fusion of those bodies. The aërolite, which, according to Schiaparelli, may in some instances be forced through our atmosphere with a relative velocity exceeding 3500 feet per second (one enormously exceeding that at which air can rush into a vacuum), compresses the stratum of air immediately in advance in almost the same manner as if at the first instant of contact the air were a rigid body. The temperature developed is greater as the velocity of compression is so, and as the volume compressed is less; the most highly heated air is therefore the stratum directly in contact at any instant with the stone; and the latter licks up more or less of the heat as it passes through a succession of such compressed strata, and so receives continual accessions of heat until the temperature of the meteoric stone itself reaches the limit given by that of the stratum of compressed air in immediate contact with it at any instant. If a body as mobile and compressible as air can thus by sufficiently rapid compression be raised above the temperature of incandescence, we may readily conceive how great an exaltation of temperature may be produced in the rigid materials of our earth's crust when exposed to a pressure which may be viewed as limitless in reference to the resistance opposed to it, and which, in consequence of the conditions of elastic resilience hereafter referred to, may give rise to motion and crushing with velocities even exceeding those with which aërolites traverse our atmosphere.

The well-known experiment of cutting a hard steel file in two by the rapid rotation of a thin disk of soft sheet iron pressed against it is another example. The heat developed at the working-point, so far as it is communicated to the disk, is rapidly

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* The observations upon which these statements are founded have been made after various great conflagrations of stores or warehouses at London, Liverpool, and Dublin, into the construction of which granite blocks and cast iron in columns, girders, &c. largely entered. The cast iron was either melted or softened to the consistence of soap; the granite heated to like temperature, except being split in various directions, was found unaltered, except more or less in colour, after having been again cooled.

carried off and dissipated by its rotation, and it thus remains cool enough to be touched by the hand, although the heat developed by it and accumulated at and near the working-point in the file is sufficient to raise that to the temperature at which cast steel becomes softened and approaches fusion.

The cutting of steel railway bars across when at a very low red heat by a rapidly revolving circular saw, which revolves partially immersed in cold water, and from whose action a torrent of incandescent fragments of steel is discharged, is a like case.

Besides the heat transformed from the work of compression and crushing, a large amount of heat must also be generally produced by transformation of the work expended in friction and detrusion. No experiments have as yet, to the author's knowledge, been made upon the amount of heat developable in fragmentary pulverulent masses, such as sand, by the forcible transposition of more or less of the particles; nor do we know with certainty the conditions under which external mechanical pressure is transmitted through sand or like discontinuous matter. As in rigid solids exposed to unequal mechanical pressures there exist planes or surfaces within the mass such as have been denominated by Moseley "planes of easiest shearing," or sliding, so in masses of pulverulent matter, whatever be the shape or size of the particles, provided these be small in relation to the whole mass, and their mutual adhesion (if any) small also, such planes must by unequal mechanical pressure be brought into existence. Along any such plane we may imagine the sand or other pulverulent matter forced to move over itself in opposite directions at opposite sides of the plane; that is to say, we may suppose the sand forced along such a plane much in the same way that a mass of sandstone or of granite would be forced along such a shearing plane as had been produced in it previously by mechanical pressure. If this reasoning be admitted, we must suppose that heat would be developed along such a plane and at short distances from it in a way more or less analogous to that produced by forcing one rough surface of stone over another. What the coefficient of friction in this case would be can only be determined by experiment; but we may justifiably conclude that the amount of friction per unit of surface would increase proportionately to the pressure applied externally to the entire mass— exposed to more or less of which, motion at any such surface of friction must take place. Coulomb, Morin, and others have found the friction of some sorts of rough stone upon other rough stone to reach as much as three fourths the pressure; and should this coefficient increase proportionately under the enormous pressures to which a discontinuous mass at several miles depth may be subjected, we can readily see that the trans-

formed heat of friction produced by internal movements taking place in such materials after crushing has occurred, must be the source of a large amount of heat over and above that originally due to the crushing itself. Thus, for example, if we assume a surface of such disintegrated material sliding over a similar surface, or over a rough surface of coherent rock, and under the pressure of ten miles of rock of the specific gravity of granite, at the rate of one foot per second, and if we take the coefficient of friction as low as 0·5, we have 4,326,600 foot-pounds of frictional work per second, which, divided by J (=772), gives 5604 units of heat evolved per second from each square foot of surface; and to this development there is no limit while the circumstances continue the same, except that of the distance that one surface is forced over the other. And great as is this evolution of heat under such enormous pressures, it would be further increased in the event of the fragmentary particles being heated so as to present incipient viscosity of surface and more or less of mutual agglutination.

Temperature with respect to any given solid material is dependent upon the units of heat present in a unit of mass or of volume of the substance. If for the same total heat we diminish the mass or volume, the temperature is proportionately increased. When the material is surrounded by matter capable of carrying off heat by conduction, or evection, or radiation, and the heat is evolved within the mass by work done upon it, then another condition, that of time, has to be taken into account; for the shorter the time within which a given amount of heat due to transformation of work is evolved within the unit of mass, the less of that total is dissipated by conduction &c.

A familiar example of this is every day seen in the light and heat elicited by abrasive friction, or collision between hard bodies. When two lumps of granite or other hard stone are struck together, heat and light are instantly evolved at the small surface of contact, where the material of one or both masses is crushed. The work done may be but that of the crushing of a fraction of a grain of the stone; but it is great in reference to the extremely brief instant of time during which the work is performed; the crushed particles are raised to a temperature of luminosity for a brief moment because there is not time for the surrounding surfaces of the cold stone to carry off the heat evolved by conduction, though the dissipation of heat thus produced is such that the luminosity again instantly disappears. The temperature at the crushing-point is greater as the work done in a unit of time and upon a given weight of the material is greater. That the temperature capable of being thus produced approaches that of the fusing-point of steel, is evident from the phenomenon of a

spark struck from the steel of a gun-lock by the flint.   In the
case of small masses of rock, such as the $1\frac{1}{2}$-inch cubes of the
author's experiments, crushed between two opposite surfaces of
steel, the actual temperature of the crushed particles can never
be found to reach that due to the work of crushing; for the heat
of the relatively small mass of the crushed cube in close contact
with far larger masses of cold steel of high conductivity is car-
ried off almost as fast as it is evolved; and as the total amount
of heat evolved from the crushing of such a cube of the hardest
rock experimented upon by the author (namely number 12,
Table I. Phil. Trans. part 1, 1873, p. 186) could only raise its
own mass through $217°$, if the temperature of fusion of the rock
may be taken at $2000°$, it is obvious that such a cube could
not be fused by the work of crushing alone, even though all the
heat due to the crushing work remained in the cube, none being
dissipated to surrounding objects.

In the case of a cube such as this losing heat by dissipation,
the temperature of the crushed mass depends upon the time in
which the work of crushing is done.   In the author's experi-
ments the crushing of each cube in column 12 occupied a mean
time somewhat greater than that in which a heavy body could
fall freely through a space of 0·09 foot (No. 12, Table I. *l. c.*, col.
19)—that is, 0·075 of a second; for more rapidly than that the
crushing surfaces could not approach each other.   If, however,
the conditions had been such that but $\frac{1}{n}$-th the above time were
expended in the crushing, then a proportionately less quantity
of the heat evolved would have been dissipated; and this, we shall
see further on, must be the case in nature.   When two rock-
surfaces are urged against each other in the shell of our globe
by the gradual withdrawal of support by contraction of the nu-
cleus, the rocky masses for great distances from the opposed
surfaces are brought into a state of elastic compression, gradually
increasing up to the crushing-point somewhere, when a greater
or less portion of rock suddenly gives way by crushing and is
more or less removed by detrusion in some lateral direction.
The material of the rock for a greater or less distance from the
crushing-point is therefore in the condition of a compressed
spring which is suddenly released.   When so released, the velo-
city of resilience depends principally upon the modulus of elas-
ticity of the rock; and the velocity of release of the spring,
which is that with which the crushing is performed, is extremely
great in the case of hard granite or generally similar homoge-
neous rock, in which it probably exceeds 10,000 feet per second,
though in very much less-elastic rocks falling considerably short

of this*. But had the rock-specimen crushed in the author's experiments been a cubic foot in place of an inch and a half cube, the time of crushing must have been *greater* than ·249 second (or nearly a quarter of a second), or the crushing would have been performed with nearly a velocity of 2 feet per second, and that would be less than $\frac{1}{5000}$ of the velocity with which the same would have been crushed if circumstanced as in the shell of our globe. And if we extend our view from the crushing of a cubic foot or two to that of a cubic mile or more, we see that there would be very little of the total heat evolved lost by dissipation, there being scarcely any time in which that could occur, the possible rate of crushing of a cubic mile being less than half a second.

In the case of a very large mass of rock crushed simultaneously, or nearly so, as every portion of the rock evolves heat proportionate to the crushing-work done upon it, so the heated portions of crushed material situated near the exterior of the entire mass act as *"jacketing,"* so as to preserve the deeper-seated portions of the heated mass almost absolutely from any dissipation of heat for a considerable length of time, that time depending, *cæteris paribus,* on the conductivity of the crushed material and the difference in temperature between the crushed material and the uncrushed rock adjacent to it. There are some experimental grounds for concluding that the absolute crushing-force per unit of volume in any given rock increases as the absolute volume of the mass simultaneously crushed becomes greater; thus it has been found by Rondelet that large cubes of stone required a proportionately larger crushing-force than smaller ones; much stress, however, cannot be laid upon this, as we cannot assume with any certainty what are the precise conditions under which rock-surfaces in our earth's shell are forced together, and the distribution of the crushing-pressure may be indefinitely varied.

In the author's experiments the cubes crushed by pressure on two opposite faces were free upon the other four; it cannot be doubted that, had only two opposite faces been free and the pressure applied simultaneously upon the four other faces, two and two respectively, the pressure necessary for crushing or the work thereof would have been considerably increased. Further, if none of the faces were free, and all those except the two opposite faces to which the crushing pressure is supposed applied, had the motion outwards of any of their particles opposed by an initial pressure, such as that of an insistent mass of rock, the work to be done to produce crushing would be necessarily in-

* See the author's " Earthquake-wave Experiments," Phil. Trans. 1862, vol. clii.

creased by the amount required to overcome such initial pressure, as has been already pointed out.

Another and further source of heat arises after crushing and detrusion of the fragmentary matter, and after the latter has arrived at a temperature at which the fragments have become more or less viscous and adherent by reason of the further work expended in the deformation and detrusion by forcing forward through highly irregular or constricted rock-channels of the heated and now viscous mass.   There do not exist at present sufficient data by which to calculate the amount of work necessary to a given amount of deformation in viscous masses; and hence we cannot calculate the amount of heat that in nature might arise from it.   Hirn, however, has shown that in the case of plastic bodies such as lead the heat developed is proportionate to the work done in deformation; so that, if we knew the pressure per unit of surface necessary to produce a certain deformation in an already heated mass of given viscosity, we could calculate how much its temperature would be exalted by the work of the assigned deformation.

Examples, however, are not wanting which prove that a very large exaltation of temperature can thus be produced—as, for example, in the old-fashioned method by which blacksmiths were accustomed to light their fires.   A thin square rod of very good tough iron was hammered at its extremity by a succession of rapidly given blows from a light hand-hammer.   After a minute or so the rod, for the portion of its length hammered at the extremity; became red, and in a second or two more of distortion of its form by continuance of the hammering, the iron could be made nearly white hot.

A similar example, on a much larger scale, occurs in the process of rolling iron or steel.   When a heavy billet of iron or steel, heated to a brilliant yellow heat, is passed between the rolls of the iron mill, and the massive lump is rapidly elongated into a bar, its temperature, notwithstanding that it is rapidly and constantly losing heat by radiation and evection, is observed visibly to increase, so that the mass becomes at a certain stage white or welding hot by the transformation into heat of the work of deformation so rapidly and powerfully applied to it.

The action of the machine employed in the arsenal at Woolwich for making lead rods to be afterwards pressed into bullets affords another striking example.   In this machine a cylindric block of lead, maintained at a temperature of 400° Fahr., is by a steady pressure upon the end, which is 8"·5 in diameter, of 16,700 lbs. per square inch of its surface, forced through an aperture at the other extremity into a rod of 0"·525 diameter, at such a rate that five inches in length of the cylindric block becomes a rod

of about 100 feet in length of the above diameter per minute. We have thus 393,906 foot-pounds of work done upon the lead per minute, dividing which by J we have 510·2 British units of heat developed per minute from the transformed work. In the actual machine the whole of this is ultimately dissipated and lost; but if none of it were dissipated, as the cylindric block of lead of 8·5 in. diameter by 0·416 ft. (5 inches) weighs 116·3 lbs., and the specific heat of lead is = 0·029 (or perhaps a little more at 400° Fahr.), it follows that the heat developed by its deformation from the short cylindric block of 5 inches length to a rod of about 100 feet length, is enough to raise the temperature of the lead through 151° Fahr., or, were no heat lost, to raise its temperature from 400° to 551° or thereabouts—that is, within about 50° of its melting point. If, therefore, we could by a reverse process squeeze the 100-foot rod back into the original block of 8″·5 × 5″, we should find the lead in the latter not only liquid, but considerably above its temperature of fusion, or at nearly 700° Fahr. It is obvious, therefore, that any viscous or plastic body such as lava, continually forced through apertures varying in area and form and suffering continual deformation, as when forced through a volcanic tube or vent, must have its temperature continually exalted so long as it continues thus to be urged forcibly forward, assuming, as is very nearly the truth in nature, that an extremely small proportion of the heat developed in the process can be dissipated by conduction to the walls of the tube.

The preceding remarks apppear to the writer sufficient to show that there is no physical difficulty in the conception involved in his original memoir (Phil. Trans. 1873), but not there enlarged upon in detail, that the temperatures consequent upon crushing the materials of our earth's crust are sufficient locally to bring these into fusion.

II. *A new Polariscope.* *By* Professor W. G. ADAMS [*].

IN devising this instrument, the principal objects in view have been :—

(1) To obtain an extensive field of view.

(2) To afford a means of measuring the rings and the angles between the optic axes of biaxal crystals.

(3) To have a means of immersing the crystal in a liquid in those cases in which the optic axes are too far apart to be seen in air.

These advantages have been obtained by modifying the positions and focal lengths of the lenses usually employed in table polariscopes, so that the rings of a crystal are best seen when there is a space of $1\frac{1}{4}$ inch between the two lenses, one on either side of the crystal. Into this space is introduced a central piece, consisting of a circular box with deep plano-convex lenses fixed, one in the bottom and the other in the top of the box, in such a position that their curved surfaces have a common centre of curvature, with their flat faces turned towards one another and enclosing the crystal between them. The box can turn about an axis passing through the common centre of curvature.

The form and position of Mirror and Lenses for the Polariscope.

A is a concave mirror about $1\frac{1}{2}$ inch in diameter, such as is ordinarily employed for illuminating microscopes, and mounted in the same manner. A double-concave lens, B, 1 inch in diameter, is placed so as to have its focus at the same point as the principal focus of the mirror. The rays coming from the mirror will then be parallel after passing through the lens. To diminish aberration as much as possible, the radius of curvature of the first face of the lens should be about six times the radius of its second face. The parallel rays then fall on a tourmaline or other polarizer, C, the diameter of which should be nearly equal to that of the lens. They then fall on a double-convex crossed lens D, the first face of the lens having the greatest curvature, so as to diminish the aberration as much as possible. This lens is 1 inch in diameter and $1\frac{1}{4}$ inch focal length.

At a distance of $\frac{1}{4}$ of an inch from this lens is placed a convexo-plane lens E, $\frac{4}{5}$ of an inch in diameter and 1 inch focal length.

These three lenses, B, D, and E, may be fixed in the same piece; and with the above arrangement, rays which are parallel before falling on the mirror will be brought to a focus at a distance of half an inch on the outside of the last lens E. The rays then fall on the central piece M N, consisting of two plano-convex lenses, which are nearly hemispheres, enclosing the crystal between them. The first of these lenses, M, is 5 millims. in thickness, and the radius of its spherical surface is 6·4 millims., or $\frac{1}{4}$ of an inch; the other lens, N, is 7 millims. in thickness, and the radius of its spherical surface is 9·6 millims., or $\frac{3}{8}$ of an inch; and they should be so placed that the two centres of curvature are accurately at the same point O. The distance between them will then be 4 millims., which is very nearly the thickness of most specimens of crystals. If lenses with the same curvatures but of thicknesses $4\frac{1}{2}$ and $6\frac{1}{2}$ millims. respectively be used in place of M and N, there will be a breadth of 5 millims. between them when they are in the best position. This central piece
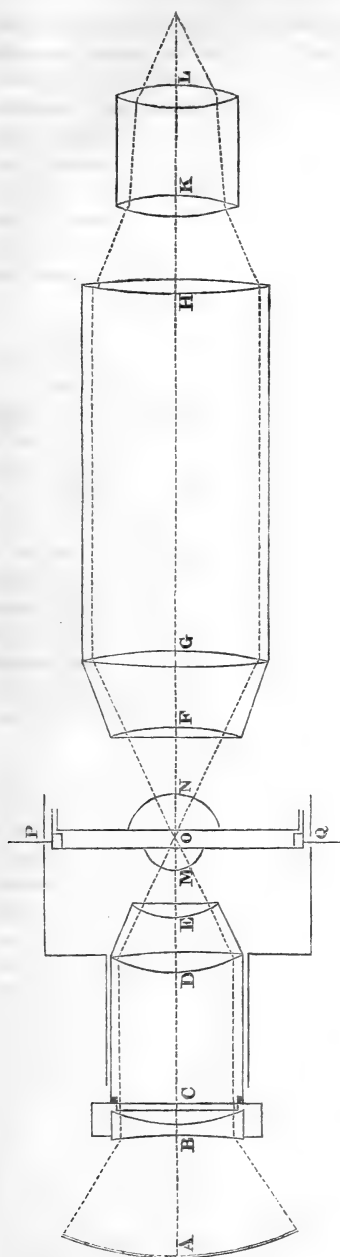
A. Concave mirror.

B. Double-concave lens 1 inch in diameter.

C. Tourmaline or other polarizer.

D. Double-convex crossed lens 1 inch in diameter, $1\frac{1}{4}$ inch focal length.

E. Convexo-plane lens $\frac{4}{5}$ inch in diameter, 1 inch focal length.

F. Plano-convex lens 1 inch in diameter, $1\frac{1}{4}$ inch focal length.

G. Double-convex crossed lens $1\frac{1}{2}$ inch in diameter, $1\frac{3}{4}$ inch focal length.

H. Double-convex lens $1\frac{1}{2}$ inch in diameter, 3 inches focal length.

K L. Nicol's prism and eyepiece.

M and N. Portions of hemispherical lenses with O as the common centre of curvature of their spherical surfaces. Radius of curvature of M is $\frac{1}{4}$ inch, and of N is $\frac{3}{8}$ of an inch, and the distance between the lenses M and N about $\frac{1}{8}$ of an inch.

P and Q. The ends of the axis supporting the box fixed so that the axis passes through the point O.

The dotted lines show the path of the light through the instrument.

should be so placed that O, the centre of curvature of the two curved surfaces of the lenses, coincides with the focus to which the rays converge after they come out of the first system of lenses. The rays will then pass through this central piece without having their direction altered, since the crystal will refract the light nearly to the same extent as glass, and the light will pass nearly as if it passed through the centre of a sphere of glass.

After passing through this central piece, the rays diverge from the common centre of curvature and fall on a plano-convex lens, F, 1 inch in diameter, and placed about $\frac{5}{8}$ of an inch from the common centre of curvature.   Its focal length should be $1\frac{1}{4}$ inch. At a distance of half an inch from this lens is another lens, G, $1\frac{1}{2}$ inch in diameter and $1\frac{3}{4}$ inch focus, so as to make the rays again parallel.   Then at a distance of 3 inches is placed a lens, H, whose focal length is rather less than 3 inches and whose diameter is $1\frac{1}{2}$ inch; and above this the Nicol's prism, K L, with any other apparatus, such as the beautiful arrangements of Mr. Spottiswoode for showing the effects of quartz and other crystals on polarized light.   At the principal focus of the last lens, H, should be placed crossed spider-lines; and for accurate measurement a simple lens or eyepiece should be added above the Nicol's prism.

The object of receiving the light from the mirror on a double-concave lens is to avoid aberration and the consequent loss of light; but this involves the use of a large piece of tourmaline. The difficulty is got over in existing polariscopes by employing two convex lenses, causing the rays to cross the axis very near the polarizing tourmaline, and then making them parallel by means of a second convex lens.   The central piece may be added to any polariscope provided there is sufficient room between the two systems of lenses to admit the crystal and two lenses, M, N, and to allow of motion about the axis through the common centre of curvature of the surfaces of these two lenses.   The two parts, B C D E and F G H, should be so arranged that when the crystal alone is placed at O the rings are best seen; then the crystal should be placed in the box between the two lenses M and N in the bottom and in the cover of the box.   This box should be supported on two wires, P and Q, forming an axis passing through O, and should be large enough to contain the cork in which the crystal is placed.   The axis is supported in a brass tube, which forms one piece with another brass tube which fits on the piece B C D E.   A semicircular disk of brass with its arc graduated is fixed on the large brass tube about the axis P as a centre; and on this axis is an index for reading-off the angle through which the axis is turned. On turning the box about the axis, no change is produced in the rays

which pass through the centre O of the curved surfaces of the lenses; but when a crystal is introduced, the rays may be sent through it in any desired direction on turning the axis, so that either of the optic axes of a biaxal crystal may be brought into the centre of the field. Thus the angle between the optic axes may be measured. The central piece M N is made in the form of a box, so that a liquid may be introduced into it for measurements in those cases in which the optic axes are too far apart to be seen in air.

The advantages to be obtained by the use of this central piece are:—

(1) The extension of the field of view. If the angle in air corresponding to the field of view is 74° without the central piece, then the angle will be increased to about 128° when the central piece is introduced, the central piece giving the same angle in glass that is given without it in air. The field of view may be made to include both optic axes of topaz of Brazil.

(2) When the plane containing the optic axes is at right angles to the axis P Q, either of the optic axes of a biaxal crystal or any ring may be brought into the centre of the field of view where the spider-lines cross one another, and the angles between them accurately measured. Instead of employing a lens for an eyepiece, the centre of the field may be determined by fixing crossed spider-lines at a point halfway between the lenses D and E. These lines will be brought to a focus by the system of lenses E, M, N, F, and G at a distance to the right of G equal to about one tenth of an inch; and at this focus another set of crossed spider-lines should be placed, so that the two sets may by their coincidence determine the centre of the field of view. The spider-lines should be in the focus of the lens H, so that they may be seen on looking through the Nicol's prism.

III. *On Nitrated Toluol.*
By Edmund J. Mills, D.Sc., F.R.S.[*]

MONONITROTOLUOL.—Toluol free from benzol, and having a nearly constant boiling-point, was nitrated by dropping into "fuming" or nitrous nitrate of sp. gr. 1·48. The product was distilled in a current of steam (an operation necessary to purify it); toluol then came over first, and afterwards a mixture of solid with liquid mononitrotoluol. The two isomers can be separated with great completeness by cooling for half an hour to −17°, and suddenly filtering with the aid of a pump. The solid modification remains on the filter; the liquid

filtrate has a constant boiling-point, and does not yield crystals
when again cooled.

I have not been able to obtain the solid product of constant
fusion-point, either by sublimation, by crystallization from spirit
after treatment with Nordhausen oil of vitriol, or by partial so-
lutions in dilute spirit.   It is essential to crystallize twice from
purified naphtha, and then from spirit of wine.   The substance
so purified, and whether derived from a varied or even inverse
method of preparation, melts at 51°·31.   This number is a mean
of the means of several sets of experiments : there were 120 ob-
servations in all ; and three thermometers were employed.

Dinitrotoluol.—The product of the action of a mixture of
hydric nitrate with hydric sulphate upon purified toluol, when
crystallized twice from naphtha and afterwards from spirit,
melted at a mean temperature of 69°·23 (84 obs., 2 thermome-
ters).   The observations were made with considerable difficulty.

The liquid mononitrotoluol above referred to is converted into
dinitrotoluol by contact with nitrous nitrate in the cold.   The
purified product melted at 69°·17 (30 obs., 2 therms.) ; but after
keeping for 19 months, the fusion-point, as ascertained by a
small number of special observations, had risen to 69°·6 nearly.

Solid nitrotoluol, when treated with nitrous nitrate on the
small scale, furnished a dinitrotoluol melting at 69°·17 (15 obs.,
1 therm.).   When eight times the previous proportions were
taken, the product fused at 69°·57 (36 obs., 1 therm.).

Trinitrotoluol.—Purified toluol was nitrated with nitrous
nitrate, and afterwards with a mixture of that nitrate with Nord-
hausen vitriol.   After crystallization from naphtha and spirit,
the product (F$_1$) had a constant fusion-point, which was not
altered by boiling for 36 hours with nitrous nitrate.   This tem-
perature also agreed with that at which an old specimen (E)
melted after fresh crystallization.   F$_1$, F$_2$, and E together furnished
as a mean result the number 78°·85 (103 obs., 2 therms.).

The liquid mononitrotoluol previously referred to as purified
by filtration at −17° and distilling, was converted into trinitro-
toluol by heating with a mixture of nitrous nitrate and Nord-
hausen vitriol.   [It may, however, be formed by an hour's ebul-
lition with a mixture of equal volumes of common hydric nitrate
and oil of vitriol.   Such ease in nitration is unusual in this
group of bodies.]   The product (K), purified as before, melted
at 78°·88 (11 obs., 1 therm.)—a result which did not invite further
investigation.

Pure solid mononitrotoluol was treated for some time with
Nordhausen vitriol and nitrous nitrate ; but the fusion-point of
the product did not become regular until the operation had been
repeated.   The substance (M) then melted at 80°·54 (70 obs.,

1 therm.) ; in crystalline form it was much more prismatic and less platy than either of the preceding.

Experiments were made as to the solubility in absolute alcohol of F_1, F_2, K, and M. One hundred parts of the solvent took up 1·540 per cent. of F_1 at 15°·3, 1·557 of F_2 at 15°·7, 1·615 of K at 15°·1, and 1·427 of M at 15°·1. Hence the error of the process (F_1 being regarded as identical with F_2) is less than ·017 on the above numbers, and K and M are distinct from each other and from F. I could hardly find any reliable difference between the solubilities of F_2 and K at about 4°·75; but K still gave the heavier residue. At 9°·6 the solubility of F_1 was 1·204 per cent. ; of M, 1·097 per cent. at 9°·3. These numbers are confirmatory of those in the first group.

Remarks.

Trinitrotoluol.—Of the three modifications above referred to, and all of which are obtained by direct processes, the one prepared from liquid mononitrotoluol requires the least expenditure of work, and has the greatest solubility. Its fusion-point is nearly identical with that prepared, though with more difficulty, from toluol itself; but this second modification shows a diminished solubility. The third modification is made from solid mononitrotoluol, with the greatest expenditure of energy ; it has an unmistakably higher fusion-point, but a lower solubility than either of the two preceding isomers.

Dinitrotoluol.—Solid mononitrotoluol is more difficult to convert into dinitrotoluol than either toluol or liquid mononitrotoluol, and melts at a distinctly higher temperature than either of them as they are when freshly prepared. Having regard, however, to the shifting fusion-point of the product from liquid nitrotoluol, the difficultly observed fusion-point of the product from toluol itself, and the impracticability of determining the solubility of either of these isomers in alcohol, I cannot quite decide whether the toluol- and liquid nitrotoluol-products are distinct. Judging from their obvious mimicry of the trinitrotoluols, I should presume that they are.

Mononitrotoluol.—The facile process of separating the liquid from the solid isomer, referred to in preceding paragraphs, will, it is hoped, save many weary fractionations. That the solid modification is produced in larger quantity when more energy is employed in the reaction, is already known as a result of Rosenstiehl's labours.

The fusion-points I have given in this paper are *good* in the second decimal place. That, however, will probably have to be lowered by a unit or two, on account of the difference between Kopp's correction and mine for the thermometers' exposure;

and all the results have still to be expressed in terms of the air-thermometer. This work is in process. In the mean time, the small but real and certain differences here exhibited, and which have cost so much rigorous effort in their attainment, illustrate the difficulties and beauties of isomerism.

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St. John's Park, N.

IV. *On the Motion of a Particle from Rest towards an Attracting Centre; Force ∝ (Distance)⁻². By* J. C. Glashan, *Esq.**

THIS problem is briefly noticed by Professor Cayley in the British Association Report, 1862, p. 186. Speaking of rectilinear motion, he says :—" The problem thus becomes a particular case of that of central forces; and it is so treated in the *Principia*, Book I. § 7; the method has the advantage of explaining the paradoxical result which presents itself in the case Force ∝ (Dist.)⁻², and in some other cases where the force becomes infinite. According to the theory, the velocity becomes infinite at the centre, but the direction of the motion is there abruptly reversed ; so that the body in its motion does not pass through the centre, but on arriving there, forthwith returns to its original position. Of course such a motion cannot occur in nature, where neither a force nor a velocity ever is actually infinite. . . . . Force ∝ (Dist.)⁻², $= \dfrac{\mu}{x^2}$, which is the case above alluded to. Assuming that the body falls from rest at a distance a, we have $x = a(1 - \cos \phi)$, where, if $n = \dfrac{a^{\frac{3}{2}}}{\sqrt{\mu}}$, ϕ is given in terms of the time by means of the equation $nt = \phi - \sin \phi$. If the body had initially a small transverse velocity, the motion would be in a very excentric ellipse ; and the formulæ are in fact the limiting form of those for elliptic motion."

In the 'Messenger of Mathematics,' N. S. vol. iii. pp. 144–149, Professor Asaph Hall sketches the history of the problem and proposes a limit-derived solution. After noticing Laplace's views he proceeds :—" We have now three different opinions with regard to the motion of the particle :—first, Professor Cayley's interpretation of Newton's investigation, which is that the particle reaches the point C [the centre of attraction][†], then moves back toward A [the original position], and continues oscillating between A and C; second, Euler's conclusion that the particle stops at the point C ; and, finally, the statement of Laplace,

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* Communicated by the Author.
† The words within the brackets are ours.

that the particle passes through the point C and oscillates between A and B" [a point equidistant with A from C].

On page 152 of the same volume of the 'Mesenger' Professor Cayley writes :—" I quite admit that, considering (with Professor Hall) the attracting particle as split into two equal particles placed at equal distances above and below the centre C, the motion when the distances become infinitesimal is a motion not as above, but backwards and forwards along the entire line AB; but it remains to be seen whether *at* the limit this can be brought out as an analytical solution of the differential equation $\dfrac{d^2x}{dt^2} = -\dfrac{\mu}{x^2}$. Possibly this may be done; and I remark as an objection, not to the foregoing as an admissible solution of the problem, but to its generality as the *only* solution, that in writing $x = a(1 - \cos \phi)$, and assuming that ϕ is real, I in effect assume that x is always positive. But the burthen of the proof is with Professor Hall, to show that there is an analytical solution in which x acquires negative values."

A problem that has received three inconsistent solutions from such mathematicians as Euler, Laplace, and Cayley is certainly worthy of notice for this alone; but this one is besides important in the theory of plane algebra, and in the theory of limits involving discontinuity.

To Professor Cayley's solution and to the last of the above quotations I object :—

(1) *The motion from absolute rest is not the limit of motion in a fixed ellipse (nor in a fixed parabola with vanishing latus rectum).*

(2) *The equation* $\dfrac{d^2x}{dt^2} = -\dfrac{\mu}{x^2}$ *does not represent the limit of motion in an ellipse; neither does it represent the motion under the conditions proposed in the problem.*

(3) *The required equation is* $\dfrac{d^2x}{dt^2} = -\dfrac{\mu}{x(+\sqrt{x^2})}$.

(4) *The problem may be considered as the limiting case of* " *a particle moved from rest under the action of a spherical surface all the parts of which attract the particle with forces varying inversely as the square of the distance, find the law of the motion of the particle*"[*].

In support of (1) it is sufficient to note that in elliptic motion, even at the limit (*elliptic* limit), both tensor and versor vary continuously and dependently, while in the case of motion from rest the versor is discontinuous, remaining absolutely constant for all variations of the vector that does not carry it through

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[*] But (?); for in this case the force at a point within the spherical surface is zero; whereas in the original problem the force continually increases and becomes infinite at the centre.—ED.

the origin, and then varying only while the tensor is *nil*; in fact here neither can vary *with* the other.   Again, an infinitesimal transverse "aphelion" motion will produce a relative-infinite transverse "perihelion" motion; but if the transverse "aphelion" motion vanish absolutely, the "perihelion" motion does so too; but it would require to be absolutely infinite were rectilinear motion the limiting case of motion in a fixed ellipse.

The truth of the first statement in (2) appears from the above, since x varies independently of the versor.   The equation defines the motion of a particle attracted while on the positive side, but repelled on the negative side of the origin.   In fact the force is *converted* by each passage of the particle through the origin.

The equation in (3) may be obtained thus:—Writing ρ (a tensor) for the length-ratio of the radius vector, ι a (primitive) 2π-root of unity for the versor, and n an integer, we have generally

$$\frac{d^2(\rho\iota^{n\pi+\theta})}{dt^2} = \frac{\mu\iota^{(n+1)\pi+\theta}}{\rho^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(A)}$$

If θ be variable this will give n constant and

$$\rho = \frac{l}{1-e\cos(\theta-a)},$$

in which l is a scalar.   The path of the attracted particle will be $(\rho\iota^{n\pi+\theta-a})$, a conic section.

If θ be constant, say $=\beta$, (A) becomes

$$\frac{d^2(\rho\iota^{n\pi+\beta})}{dt^2} = \frac{\mu\iota^{(n+1)\pi+\beta}}{\rho^2}, \quad . \quad . \quad . \quad . \quad \text{(B)}$$

which reduces to

$$\frac{d^2x}{dt^2} = -\frac{\mu}{x(+\sqrt{x^2})}. \quad . \quad . \quad . \quad . \quad . \quad \text{(C)}$$

Applying Professor Cayley's solution to (B), we get

$$\rho = a(1-\cos\phi),$$

$$\phi - \sin\phi = \frac{t\sqrt{\mu}}{a^{\frac{3}{2}}};$$

and the path of the particle to be $\rho\iota^{n\pi+\beta}$.   This path, interpreted in the usual way for a radius vector passing through the origin, gives for the particle an oscillatory motion through the centre of attraction; n will here be a discontinuous variable.

I have used the above forms of (A) and (B) for the sake of clearness in the dynamics; but analytically there is neither break nor distinction.

$$\frac{d^2(\rho\iota^\theta)}{dt^2} = \frac{\mu\iota^{\pi+\theta}}{\rho^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \text{(D)}$$

$$\therefore \ \rho\{1 - e\cos(\theta - a)\} = l,$$

in which l may contain the versor $\iota^{n\pi}$. The figure is $\rho\iota^{n\pi+\theta-a}$, a conic section or straight line according as l is actual or *nil*. Taking the case of the ellipse and tracing its change of form as l varies from positive through zero to negative, we have, fig. 1, l finite and positive; fig. 2, limit *to* which fig. 1 tends as l approaches zero; fig. 3, limit *from* which fig. 4 tends as l draws away from zero negatively; fig. 4, l finite and negative. The
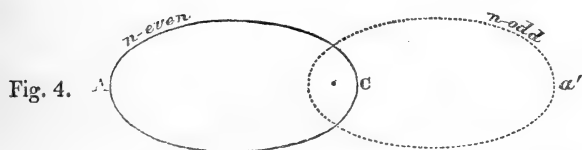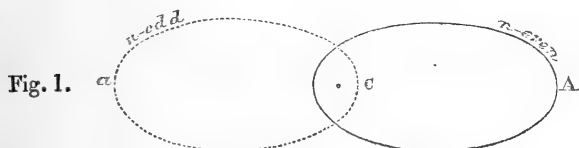
passage from fig. 2 to fig. 3 occurs discontinuously at $\rho = 0$. (In the plane sections of the cone, figs. 2 and 3 occur simultaneously, so that a line from A to A$'$ appears in the primary plane, and one from a to a' in the secondary. There is no discontinuity in the motion of the section-plane, the analytical discontinuity merely indicating that the passage from the lower to the upper *semi-cone* is instantaneous, but through a position in which *both the semi-cones* are cut.)

For the particle to change from an n-even to an n-odd line would require discontinuity of motion; but in the problem there is no force to cause this discontinuity; for it would needs occur at the centre, and there the attraction is *nil*. The motion will

lie wholly in the primary plane; *and its actual section of the* complete cone *at the limit is seen in* fig. 2 *a*.

To sum the whole argument, Professor Cayley's solution is based on, "what is true up to the limit will be true at the limit;" but he has overlooked the condition, "provided there be no discontinuity at the limit." In the passage of an ellipse with positive radius vector through a parabola with vanishing latus rectum to an ellipse with negative radius vector there is discontinuity; yet it is thus that elliptic motion from positive transverse initial motion passes to elliptic motion from negative transverse initial motion. The difficulty arises from the discontinuity in direction of the transverse initial motion in its passage through zero.

No remarks on (4) are needed, further than that it states one of the *many* problems of which rectilinear motion through an attracting centre is the limit. I prefer it on account of the investigation by means of the hodograph. An examination by that method of the various proposed limit-problems and solutions will well repay making it.

Turning now to the equation $\dfrac{d^2x}{dt} = -\dfrac{\mu}{x^2}$, I find it uniformly applied to the above-discussed problem, even by those who accept Laplace's views of the particle's motion. But here they are inconsistent; for Professor Cayley's is not merely "an admissible solution" of the equation, it is the *only* one. Negative values of *x* cannot be expressed in actual (*non-nil*) values of *t*. *Either reject the equation or accept the solution.* Here it is a case of pure analysis; and in such no man need attempt to glean after Professor Cayley. The equation may be considered to express the limiting case of, "A particle moves from rest under the action of a spherical surface, all the parts of which at first attract the particle with forces varying inversely as the square of the distance, but {each} passage of the particle through the centre of the spherical surface converts the attraction into repulsion {or *vice versâ*}; find the law of the particle's motion."

Strathroy, Ontario, May 15, 1875.

V. *An Experiment for showing the Electric Conductivity of various forms of Carbon. By* H. Bauerman, *F.G.S.**

THE following simple method of exhibiting the conducting-power of carbon was brought to my notice by my friend Mr. W. J. Ward, of the Metallurgical Laboratory of the Royal

School of Mines, as having been shown to him several years since by Dr. von Kobell, of Munich. As I have not found any account of it published, I have ventured to bring it before this Society.

A fragment of the substance to be tested, whether charcoal, coke, anthracite, or other form of carbon, is held between the jaws of a pair of tongs formed by bending a strip of sheet zinc into a horseshoe form, and immersed in a solution of cupric sulphate. If the carbon is a non-conductor, the copper salt is decomposed, and deposit of copper only takes place on the immersed surface of the zinc; but when it possesses a high degree of conductivity a zinc-carbon couple is formed, and deposit of copper takes place on the surface of the carbon as in ordinary electrotyping.

Of the different forms of carbon experimented upon, the most rapid results have been obtained with some American anthracites, and coals that have been subjected to the action of intruded igneous rocks. The most remarkable of these is an anthracite from Peru, which contains a large amount of sulphur in organic combination, and is found in a nearly vertical position, interstratified in quartzite, in the high plateau of the Andes, about 13,000 feet above the sea-level, near Truxillo. It is probably of secondary age, the metamorphism having taken place at the time of the great trachytic outbursts which form the gold- and silver-bearing rocks of the adjacent mining-district. This is coppered by immersion almost as readily as graphite. The anthracite of Pennsylvania possesses the same property, but not in quite such a high degree. The Heathen coal of South Staffordshire, when altered by the intrusion of the "white-rock" trap, is more slowly coppered; but this is probably due to the resistance interposed by the numerous laminæ of calcite filling the fractures in the mass of the coal, which renders the conductivity less perfect. A specimen of coal from Bengal, altered in the same manner by intrusion of igneous rock, behaves much in the same way as coke, being coppered directly. This is rather remarkable, as this coal is a very impure one, and contains such a large quantity of water very intimately combined, probably as a hydrated silicate interspersed through the mass, as to decrepitate explosively when suddenly heated.

The ordinary Welsh anthracite does not appear to be a conductor by this method; but after having been heated to a full red heat it conducts electricity freely. The lowest temperature at which this change takes place appears to be somewhere between the melting-points of zinc ($430°$ C.) and silver ($1000°$ C.), as fragments of anthracite packed in a thin clay crucible and plunged into molten zinc were not found to be altered, but were

changed when heated in a bath of melted silver. These limits, although considerably wide apart, are interesting as giving a possible clue to the temperature at which anthracitic metamorphism of coals has been effected in different districts. Mr. W. C. Roberts has recently shown that the alloys of silver and copper have very definite melting-points; it will be possible therefore to determine more nearly the lowest temperature necessary to produce the change.

In the South Wales anthracite district it is well known that no great amount of disturbance has taken place in the position of the coal-seams, while in North America and Peru the change has been accompanied with much more violent action, as evidenced by the greater disturbance of the rocks; and probably a correspondingly higher degree of heat was developed in the mass. The evidence afforded by the coals that have been actually altered by intruded rocks, and must have been highly heated, appears to bear out this view. On the other hand, long-continued exposure to a lower temperature might possibly produce the same effect, and further experiments upon this point would be desirable.

VI. *On the Theorem of the Mean Ergal, and its Application to the Molecular Motions of Gases.* By R. CLAUSIUS[*].

§ 1. IN a memoir published in 1870[†], for the elucidation of the second proposition of the mechanical theory of heat, I employed an equation which referred to a motion of a material point in a closed path; and in one which appeared in 1873[‡], I advanced an equation of greater generality, expressing a new and, as I believe, more fruitful proposition in relation to stationary motions.

Given any system whatever of material points which, under the influence of forces that possess a force-function or ergal, move in a stationary manner. In order to determine the position taken by the points at the time t, rectangular coordinates, or polar coordinates, or other suitable quantities can be employed. We will admit universally for this purpose the introduction of arbitrary variables, which may be denoted by $q_1, q_2, \ldots q_n$. Then the ergal U is also to be considered as a function of these variables. Further, according to Lagrange, the *vis viva* T of the

system can be represented by a function of these variables and their differential coefficients, taken according to the time, which we will designate by $q'_1, q'_2, \ldots q'_n$, which function is homogeneous of the second degree, and therefore satisfies the equation

$$2\mathrm{T} = \frac{d\mathrm{T}}{dq'_1} q'_1 + \frac{d\mathrm{T}}{dq'_2} q'_2 + \ldots + \frac{d\mathrm{T}}{dq'_n} q'_n.$$

If, for shortness, we put

$$p_\nu = \frac{d\mathrm{T}}{dq'_\nu}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad ; \quad (1)$$

ν denoting any one of the indices from 1 to n, the equation becomes

$$2\mathrm{T} = p_1 q'_1 + p_2 q'_2 + \ldots + p_n q'_n,$$

or, employing the sign of summation,

$$2\mathrm{T} = \Sigma p q'. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

We may now suppose that another, infinitesimally different stationary motion takes the place of the one previously considered. The difference may be occasioned either by the initial positions and velocities of the points not being precisely identical with those in the original motion, or by the function U, representing the ergal, having a slightly different form. In order to express the latter more simply, we will assume that the function U contains, besides the variables $q_1, q_2, \ldots q_n$, also one or more quantities which are constant during each of the two motions, but have not quite the same values in the one motion as in the other. The primitive values may be designated by c_1, c_2, &c., and the changed values by $c_1 + \delta c_1, c_2 + \delta c_2$, &c.

If we now suppose the mean value of the ergal to be formed during each of the two motions, one of these two mean values is somewhat different from the other; and we call their difference the variation of the mean ergal. It is this variation which is to be determined by my equation, as it is brought into relation with other variations.

To make the equation as readily intelligible as possible, it shall be first presupposed that both in the original and in the altered motion the variables $q_1, q_2, \ldots q_n$ accomplish their variations periodically. We will write $i_1, i_2, \ldots i_n$ for the time-intervals which serve as the periods of the individual variables in the original motion, and $i_1 + \delta i_1, i_2 + \delta i_2, \ldots i_n + \delta i_n$ for the same in the altered motion. Further, of every variable quantity in the course of the motion, we will distinguish the mean value from the variable values by putting a horizontal line over the symbol.

My equation then reads,

$$\delta(\overline{U} - \overline{T}) = \Sigma \overline{pq'} \delta \log i + \Sigma \frac{\overline{dU}}{dc} \delta c, \quad . \quad . \quad . \quad \text{(I.)}$$

in which the first summation on the right-hand side refers to all the n variables q, and, consequently, to all the n intervals i, and the second summation applies to the above-mentioned constants occurring in the ergal.

In my previous memoir I have further shown that this equation not merely holds good when the quantities $q_1, q_2, \ldots q_n$ occur periodically, but is also applicable to other stationary motions, if the time-intervals $i_1, i_2, \ldots i_n$ can be chosen so as to satisfy a certain condition-equation there given, into the consideration of which, however, I shall not here enter, as it would require explanations which are not needed for the understanding of what follows.

§ 2. To the preceding equation other forms can be given which are theoretically interesting and also convenient to use.

From equation (2) we get

$$2\delta\overline{T} = \Sigma\delta\overline{pq'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(3)}$$

If this equation be added to (I.), and E be put for the sum $\overline{U} + \overline{T}$, which denotes the energy of the system, we then obtain

$$\delta E = \Sigma \frac{1}{i} \delta(\overline{pq'}i) + \Sigma \frac{\overline{dU}}{dc} \delta c, \quad . \quad . \quad . \quad \text{(II.)}$$

or, in another form,

$$\delta E = \Sigma \overline{pq'} \delta \log(\overline{pq'}i) + \Sigma \frac{\overline{dU}}{dc} \delta c. \quad . \quad \text{(IIA.)}$$

Dividing equation (3) by 2, and then adding it to (I.), gives

$$\delta\overline{U} = \frac{1}{2} \Sigma \frac{1}{i^2} \delta(\overline{pq'}i^2) + \Sigma \frac{\overline{dU}}{dc} \delta c, \quad . \quad . \quad \text{(III.)}$$

or, in another form,

$$\delta\overline{U} = \frac{1}{2} \Sigma \overline{pq'} \delta \log(\overline{pq'}i^2) + \Sigma \frac{\overline{dU}}{dc} \delta c. \quad . \quad \text{(IIIA.)}$$

In equation (I.) the quantity $\overline{U} - \overline{T}$ is to be considered as a function of the various intervals i and constants c; and the equation can be analyzed into as many partial equations as there occur independent variations on the right-hand side. Just so in equations (II.) and (IIA.) the energy is to be looked upon as a function of the several quantities $\overline{pq'}i$ and the constants c, and, finally, in equations (III.) and (IIIA.) the mean ergal $\overline{U}$ as a function of the quantities $\overline{pq'}i^2$ and the constants c.

For shortness' sake we will introduce single letters instead of $\overline{pq'}\,i$ and $\overline{pq'}\,i^2$, putting

$$c_\nu = \overline{p_\nu q'_\nu}\, i_\nu \text{ and } u_\nu = \overline{p_\nu q'_\nu}\, i_\nu^2. \quad . \quad . \quad . \quad . \quad (4)$$

Equations (II.), (IIA.), (IIIA.) then change into

$$\delta E = \Sigma \frac{1}{i}\, \delta e + \Sigma \frac{\overline{dU}}{dc}\, \delta c, \quad . \quad . \quad . \quad . \quad \text{(IIB.)}$$

$$\delta E = \Sigma \overline{pq'} \delta \log e + \Sigma \frac{\overline{dU}}{dc}\, \delta c, \quad . \quad . \quad . \quad \text{(IIc.)}$$

$$\delta \bar{U} = \frac{1}{2} \Sigma \frac{1}{i^2}\, \delta u + \Sigma \frac{\overline{dU}}{dc}\, \delta c, \quad . \quad . \quad . \quad \text{(IIIB.)}$$

$$\delta \bar{U} = \frac{1}{2} \Sigma \overline{pq'} \delta \log u + \Sigma \frac{\overline{dU}}{dc}\, \delta c. \quad . \quad . \quad \text{(IIIc.)}$$

Since the occurrence of the quantity $\bar{U}$ is characteristic of these equations, we will name the theorem which is expressed, only in different forms, by each of them *the theorem of the mean ergal.*

§ 3. As already said, each of the equations (I.), (II.), and (III.) can be analyzed into as many partial equations as there are independent variations on the right-hand side. We can, for example, write equation (IIIc.) in the following form :—

$$\frac{d\bar{U}}{du_1}\delta u_1 + \frac{d\bar{U}}{du_2}\,\delta u_2 + \ldots + \frac{d\bar{U}}{du_n}\,\delta u_n + \frac{d\bar{U}}{dc_1}\,\delta c_1 + \frac{d\bar{U}}{dc_2}\,\delta c_2 + \&c.$$

$$= \frac{1}{2}\overline{p_1 q'_1}\frac{\delta u_1}{u_1} + \frac{1}{2}\overline{p_2 q'_2}\frac{\delta u_2}{u_2} + \ldots + \frac{1}{2}\overline{p_n q'_{n'}}\frac{\delta u_n}{u_n}$$

$$+ \frac{\overline{dU}}{dc_1}\,\delta c_1 + \frac{\overline{dU}}{dc_2}\,\delta c_2 + \&c. \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

If now we assume that all the variations are independent of one another, we can suppose the factors of each variation standing on both sides equal to one another. Applying this, first, to the variations δu, we get n equations of the form

$$\frac{d\bar{U}}{du_\nu} = \frac{1}{2}\overline{p_\nu q'_\nu}\frac{1}{u_\nu},$$

or,

$$\frac{d\bar{U}}{du_\nu}u_\nu = \frac{d\bar{U}}{d\log u_\nu} = \frac{1}{2}\overline{p_\nu q'_\nu}. \quad . \quad . \quad . \quad (6)$$

Therefore *the differential coefficient of the mean ergal according to the logarithm of a* u *is equal to the part in question of the* vis viva. Equations (I.) and (II.) can be treated in like manner; and

we thereby get, in addition to the foregoing, the following equations :—

$$\frac{1}{2}\frac{d(\overline{U}-\overline{T})}{di_\nu}i_\nu = \frac{1}{2}\frac{dE}{de}e_\nu = \frac{d\overline{U}}{du_\nu}u_\nu = \frac{1}{2}\overline{p_\nu q'_\nu}. \quad . \quad (6\,a)$$

I will here mention that in a recent memoir* I have developed the equation

$$\frac{1}{2}\overline{p_\nu q'_\nu} = \frac{1}{2}\frac{d(U-T)}{dq_\nu}q_\nu + \frac{1}{2}\frac{d(p_\nu q_\nu)}{dt}.$$

Now, if the variables $q_1, q_2, \ldots q_n$ are of such a nature that they can be unambiguously determined from the position of the points, the mean value of the differential coefficient $\dfrac{d(p_\nu q_\nu)}{dt}$ in a stationary motion is to be considered a vanishing quantity; hence, if the mean values be taken, the equation will become

$$\frac{1}{2}\overline{p_\nu q'_\nu} = \frac{1}{2}\overline{\frac{d(U-T)}{dq_\nu}}q_\nu. \quad . \quad . \quad . \quad . \quad (7)$$

The expression here standing on the right-hand side is the virial referred to the variable q_ν.

Now, in equations $(6\,a)$, three other expressions occur which are also equal to $\dfrac{1}{2}\overline{p_\nu q'_\nu}$, and can therefore be regarded as expressions of the virial. Besides it is to be remarked that these latter are more convenient to use, inasmuch as they are products which can be at once analyzed into two factors; while, the expression in (7) being the mean value of the product of two variables, such an analysis of it cannot be effected.

If, in equation (5), we consider the terms which are affected by the variations $\delta c_1, \delta c_2$, &c., understanding by μ one of the indices 1, 2, &c., we can derive partial equations of the following form :—

$$\frac{d\overline{U}}{dc_\mu} = \overline{\frac{dU}{dc_\mu}}. \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Here the extremely slight difference, that the horizontal stroke stands on the left side over the U only, and on the right side over the entire differential coefficient, makes an important difference in the signification, and the meaning of the equation can be expressed as follows :—*When the mean ergal is considered as a function of* $u_1, u_2, \ldots u_n, c_1, c_2,$ &c., *and these are differentiated according to* $c_\mu,$ *we get the same quantity as when we differentiate*

* Pogg. *Ann.* Jubelband, p 411, equation (40); Phil. Mag. S. 4. vol. xlviii. p. 1 (equation 40, p. 9).

according to c_μ *the variable ergal represented by* q_1, q_2, ... q_n, c_1, c_2, &c. *and take the mean value of this differential coefficient.*

In a corresponding manner, from equations (I.) and (II.) are obtained partial equations of the form:—

$$\frac{d\mathrm{E}}{dc_\mu} = \frac{\overline{d\mathrm{U}}}{dc_\mu}, \quad \cdots \quad (8a)$$

$$\frac{d(\overline{\mathrm{U}} - \overline{\mathrm{T}})}{dc_\mu} = \frac{\overline{d\,\mathrm{U}}}{dc_\mu}, \quad \cdots \quad (8b)$$

in which E is to be considered as a function of e_1, e_2, ... e_n, c_1, c_2, &c., and $\overline{\mathrm{U}} - \overline{\mathrm{T}}$ as a function of i_1, i_2, ... i_n, c_1, c_2, &c.

§ 4. As an example for the application of the theorem of the mean ergal, in my former memoir I treated of the motion of two material points under the influence of their reciprocal attraction. Here we will first consider another case, which, on account of its great simplicity, is well suited to make the thing evident.

Given a material point with the mass m, its position determined by rectangular coordinates which we will designate by x_1, x_2, and x_3. On this point a force acts, the components of which, taken along the directions of coordinates, X_1, X_2, and X_3 are proportional to an odd positive power of the coordinates, so that, if n is a positive even number, we can put

$$\mathrm{X}_1 = -n\frac{x_1^{n-1}}{c_1^n}, \quad \mathrm{X}_2 = -n\frac{x_2^{n-1}}{c_2^n}, \quad \mathrm{X}_3 = -n\frac{x_3^{n-1}}{c_3^n}, \quad \cdot \quad \cdot \quad (9)$$

where c_1, c_2, c_3 represent three positive constants; and accordingly we can form for the ergal the equation

$$\mathrm{U} = \left(\frac{x_1}{c_1}\right)^n + \left(\frac{x_2}{c_2}\right)^n + \left(\frac{x_3}{c_3}\right)^n. \quad \cdots \quad (10)$$

As in this case each component of the force depends only on the coordinate which belongs to it, and not on the other two coordinates, we can consider the motion in each coordinate-direction separately. The part of the *vis viva* which refers to a single coordinate-direction is $\frac{m}{2}\left(\frac{dx}{dt}\right)^2$ or, written differently, $\frac{m}{2}x'^2$; and the portion of the ergal referring to the same coordinates we will designate by H, so that we can put

$$\mathrm{H} = \left(\frac{x}{c}\right)^n. \quad \cdots \quad (11)$$

Accordingly equation (I.), if referred only to the motion in the

one coordinate-direction, takes the form

$$\delta\left(\overline{H}-\frac{m}{2}\,\overline{x'^2}\right)=m\overline{x'^2}\,\delta\log i+\frac{d\overline{H}}{dc}\,\delta c. \quad . \quad . \quad (12)$$

Further, according to the theorem of the virial,

$$\frac{m}{2}\,\overline{x'^2}=-\frac{1}{2}\,\overline{Xx}=\frac{n}{2}\overline{\left(\frac{x}{c}\right)^n}.$$

Combining this with (11), we get

$$\overline{H}=\frac{m}{n}\,\overline{x'^2}*, \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$\frac{d\overline{H}}{dc}=-n\,\frac{\overline{x^n}}{c^{n+1}}=-m\overline{x'^2}\,\frac{1}{c}. \quad . \quad . \quad . \quad (14)$$

By insertion of these values equation (12) is changed into

$$-\frac{n-2}{2n}\,\delta\,(m\overline{x'^2})=m\overline{x'^2}\,\delta\log i-m\overline{x'^2}\,\delta\log c,$$

which equation, after putting

$$\frac{n-2}{2n}\,\delta(m\overline{x'^2})=\frac{n-2}{2n}\,m\overline{x'^2}\,\delta\log\,(m\overline{x'^2})=m\overline{x'^2}\,\delta\log\,(m\overline{x'^2})^{\frac{n-2}{2n}},$$

can be brought into the form

$$\delta\log\left[\frac{i}{c}(m\overline{x'^2})^{\frac{n-2}{2n}}\right]=0; \quad . \quad . \quad . \quad (15)$$

and from this it follows that

$$\frac{i}{c}\,(m\overline{x'^2})^{\frac{n-2}{2n}}=C, \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

in which C is a constant the value of which depends on n.

This equation can be written thus,

$$i=Cc(m\overline{x'^2})^{\frac{2-n}{2n}};$$

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* I here take the opportunity to remark that the relation expressed in (13), between the mean ergal and the mean *vis viva,* has a much more general validity. If, namely, for any system whatever of material points the ergal (abstraction made of the arbitrary additive constants, which can be supposed equal to *nil*) can be represented by a homogeneous function of the coordinates of the points, then (n signifying the degree of the homogeneous function) *the virial, and consequently also the mean* vis viva, *is equal to the mean ergal multiplied by* $\frac{n}{2}$.

and from it we can at once derive the following equations—

$$e = m\overline{x'^2}i = Cc(m\overline{x'^2})^{\frac{n+2}{2n}},$$

$$u = m\overline{x'^2}i^2 = C^2c^2(m\overline{x'^2})^{\frac{2}{n}};$$

from which, conversely, the following equations also can be formed—

$$m\overline{x'^2} = \left(\frac{Cc}{i}\right)^{\frac{2n}{n-2}} = \left(\frac{e}{Cc}\right)^{\frac{2n}{n+2}} = \left(\frac{u}{C^2c^2}\right)^{\frac{n}{2}}. \quad (17)$$

From these, taking into consideration (13), result :—

$$\left.\begin{array}{l}
\overline{H} - \dfrac{m}{2}\,\overline{x'^2} = \dfrac{2-n}{2n}m\overline{x'^2} = \dfrac{2-n}{2n}\left(\dfrac{Cc}{i}\right)^{\frac{2n}{n-2}}; \\[2ex]
\overline{H} + \dfrac{m}{2}\,\overline{x'^2} = \dfrac{n+2}{2n}m\overline{x'^2} = \dfrac{n+2}{2n}\left(\dfrac{e}{Cc}\right)^{\frac{2n}{n+2}}; \\[2ex]
\overline{H} = \dfrac{1}{n}\,m\overline{x'^2} = \dfrac{1}{n}\left(\dfrac{u}{C^2c^2}\right)^{\frac{n}{2}}.
\end{array}\right\} \quad (18)$$

Equations of this form are valid for all three coordinate-directions ; and when we form the sum of each three equations belonging to one another, at the same time designating the total *vis viva* by T, and the total ergal by U, and the total energy by E, we obtain :—

$$\left.\begin{array}{l}
\overline{U} - \overline{T} = \dfrac{2-n}{2n}\,C^{\frac{2n}{n-2}}\left[\left(\dfrac{c_1}{i_1}\right)^{\frac{2n}{n-2}} + \left(\dfrac{c_2}{i_2}\right)^{\frac{2n}{n-2}} + \left(\dfrac{c_3}{i_3}\right)^{\frac{2n}{n-2}}\right]; \\[2ex]
E = \dfrac{n+2}{2n}\,C^{-\frac{2n}{n+2}}\left[\left(\dfrac{e_1}{c_1}\right)^{\frac{2n}{n+2}} + \left(\dfrac{e_2}{c_2}\right)^{\frac{2n}{n+2}} + \left(\dfrac{e_3}{c_3}\right)^{\frac{2n}{n+2}}\right]; \\[2ex]
\overline{U} = \dfrac{1}{n}\,C^{-n}\left[\left(\dfrac{u_1}{c_1^2}\right)^{\frac{n}{2}} + \left(\dfrac{u_2}{c_2^2}\right)^{\frac{n}{2}} + \left(\dfrac{u_3}{c_3^2}\right)^{\frac{n}{2}}\right].
\end{array}\right\}(19)$$

§ 5. As a further example we will discuss a case which we shall be able to make use of subsequently, in the investigation concerning the molecular motion of gaseous bodies.

Namely, let us suppose that a material point moves in a rectangular parallelepiped-shaped vessel, by the sides of which it is repelled. The force exerted upon the point by one of the walls shall depend only on its distance from that wall, and be proportional to a negative power of the distance. It may further be supposed that the force diminishes so rapidly as the distance

increases that it is sensible only in proximity to the side ; while it may be neglected at greater distances, and especially in the centre of the parallelepipedal space.

In order to determine the position of the point, let rectangular coordinates x_1, x_2, x_3, be introduced, the origin of which coincides with the centre of the parallelepipedon, and their directions are parallel to its edges.

If we now consider any one of the coordinates (which may be designated by x without index), and if c denote the distance of the two sides perpendicular to this coordinate's direction from the centre, the distances of the movable point from these two sides will be $c-x$ and $c+x$.   The forces exerted by the two sides upon the point, of which the first acts along the negative and the second along the positive x-direction, we will represent by

$$-m\frac{n\alpha^n}{(c-x)^{n+1}} \text{ and } m\frac{n\alpha^n}{(c+x)^{n+1}},$$

m denoting the mass of the material point, n a positive number, and α a constant very small in comparison with c.   We hence obtain, for the total force-component X acting along the x-direction on the point, the equation

$$X = m\left[-\frac{n\alpha^n}{(c-x)^{n+1}} + \frac{n\alpha^n}{(c+x)^{n+1}}\right]. \quad . \quad . \quad . \quad (20)$$

This gives, for the part of the ergal that refers to the x-direction, the expression

$$m\left[\frac{\alpha^n}{(c-x)^n} + \frac{\alpha^n}{(c+x)^n}\right].$$

Since, according to (20), the force-component belonging to one coordinate-direction depends only on that coordinate and not on the two others, we can again, as in the former case, consider the motion in each of the directions separately.   A further simplification results from the fact that, on account of the smallness of the factor α^n with every position of the point, we need take into account only one of the two terms in the brackets on the right-hand side of equation (20), viz. that one of which the denominator is less than c^{n+1}.   If, then, we wish preliminarily to limit our consideration to the motion during a period in which x has only positive values, for this portion of the motion we can employ the following equation :

$$X = m\frac{d^2x}{dt^2} = -m\frac{n\alpha^n}{(c-x)^{n+1}},$$

from which comes

$$\frac{1}{2}\left(\frac{dx}{dt}\right)^2 = k - \frac{\alpha^n}{(c-x)^n}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (21)$$

k being a constant the meaning of which is evident from this, that mk represents the portion of the energy referred to the x-direction. This equation we can bring into the following form :—

$$dt = \frac{dx}{\sqrt{2}\sqrt{k - \dfrac{\alpha^n}{(c-x)^n}}}. \quad \cdot \quad \cdot \quad \cdot \quad (22)$$

In order, further, to determine from this the time required for the point, moving from a place where $x=0$, to arrive at the place near the positive boundary-plane where $\dfrac{dx}{dt}=0$ and where it reverses the direction of its motion, we have to integrate this expression of the time-differential from $x=0$ to $x=a$, when a denotes that value of x for which the equation

$$k - \frac{\alpha^n}{(c-a)^n} = 0 \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (23)$$

holds good.

To effect the integration let us develop into a series the expression on the right side of equation (22), thus—

$$dt = \frac{dx}{\sqrt{2k}}\left[1 + \frac{1}{2}\frac{\alpha^n}{k(c-x)^n} + \frac{1\cdot 3}{2\cdot 4}\frac{\alpha^{2n}}{k^2(c-x)^{2n}} + \cdots\right].$$

Integrating this expression from $x=0$ to $x=a$, and denoting the time thence obtained by $\dfrac{1}{4}i$, because it is one fourth of that which the point occupies in going and returning once between the two sides, we get

$$\frac{1}{4}i = \frac{1}{\sqrt{2k}}\left\{a + \frac{1}{2}\frac{\alpha^n}{(n-1)k(c-a)^{n-1}} - \frac{1}{2}\frac{\alpha^n}{(n-1)kc^{n-1}}\right.$$
$$\left. + \frac{1\cdot 3}{2\cdot 4}\frac{\alpha^{2n}}{(2n-1)k^2(c-a)^{2n-1}} + \cdots - \frac{1\cdot 3}{2\cdot 4}\frac{\alpha^{2n}}{(2n-1)k^2c^{2n-1}} - \cdots\right\}$$

If in this we neglect the terms which have the minus sign, on account of the smallness of the fraction $\dfrac{a}{c}$, and, in conformity with (23), in the other terms put

$$c - a = \alpha k^{-\frac{1}{n}} \text{ and } a = c - \alpha k^{-\frac{1}{n}},$$

we get

$$\frac{1}{4}i = \frac{1}{\sqrt{2k}}\left\{c - \alpha k^{-\frac{1}{n}}\left[1 - \frac{1}{2(n-1)} - \frac{1.3}{2.4(2n-1)} - \cdots\right]\right\}. \quad (24)$$

Multiplying equations (21) and (22) together, we obtain

$$\frac{1}{2}\left(\frac{dx}{dt}\right)^2 dt = \frac{dx}{\sqrt{2}}\sqrt{k - \frac{\alpha^n}{(c-x)^n}}.$$

If this equation be treated in the same way as (22), taking into consideration that we can put

$$\frac{1}{2}\int_0^{\frac{1}{4}i}\left(\frac{dx}{dt}\right)^2 dt = \frac{1}{2}\left(\frac{dx}{dt}\right)^2 \frac{1}{4}i,$$

and, for abbreviation, we introduce w with the signification

$$w = \frac{1}{2}\left(\frac{dx}{dt}\right)^2, \quad \cdots \quad \cdots \quad (25)$$

we arrive at the following one:—

$$\frac{1}{4}wi = \sqrt{\frac{k}{2}}\left\{c - \alpha k^{-\frac{1}{n}}\left[1 + \frac{1}{2(n-1)} + \frac{1.1}{2.4(2n-1)}\right.\right.$$
$$\left.\left. + \frac{1.1.3}{2.4.6(3n-1)} + \cdots\right]\right\}. \quad \cdots \quad (26)$$

As, finally, if we denote by mh the mean value of that part of the ergal which relates to the x-direction, we may put $h = k - w$, we obtain from (24) and (26):—

$$\frac{1}{4}hi = \frac{1}{\sqrt{2}}\alpha k^{\frac{n-2}{2n}}\left[\frac{1}{n-1} + \frac{1}{2(2n-1)} + \frac{1.3}{2.4(3n-1)} + \cdots\right]. \quad (27)$$

To simplify equations (24), (26), and (27), we will substitute a single letter for the series which occurs in (26), putting

$$R = 1 + \frac{1}{2(n-1)} + \frac{1.1}{2.4(2n-1)} + \frac{1.1.3}{2.4.6(3n-1)} + \cdots \quad (28)$$

When this equation is multiplied by $\frac{n-2}{n}$ and then the following quantity developed in the form of a series,

$$\frac{2}{n}\sqrt{1-1} = \frac{2}{n}\left(1 - \frac{1}{2} - \frac{1.1}{2.4} - \frac{1.1.3}{2.4.6} - \cdots\right),$$

which is $= 0$, is added, we get

$$\frac{n-2}{n}R = 1 - \frac{1}{2(n-1)} - \frac{1.3}{2.4(2n-1)} - \frac{1.3.5}{2.4.6(3n-1)} - \cdots,$$

the series which occurs in (24). From this it results simulta-

neously that the series in (27), which is the difference of the two others, can be represented by

$$\mathrm{R} - \frac{n-2}{n}\,\mathrm{R} = \frac{2}{n}\,\mathrm{R}.$$

Inserting these expressions for the respective series, we will introduce a single symbol for the product $a\mathrm{R}$ which occurs in all three equations, by putting

$$\beta = a\mathrm{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (29)$$

The equations thence arising, after being multiplied by 4, are:—

$$\left.\begin{array}{l} i = 2\sqrt{\dfrac{2}{k}}\left(c - \dfrac{n-2}{n}\,\beta k^{-\frac{1}{n}}\right); \\[2ex] wi = 2\sqrt{2k}\,(c - \beta k^{-\frac{1}{n}}); \\[2ex] hi = \dfrac{4}{n}\sqrt{2}\,\beta k^{\frac{n-2}{2n}}. \end{array}\right\} \quad . \quad . \quad . \quad (30)$$

§ 6. We will now employ these equations in order to represent the quantity $h - w$ as a function of i, the quantity k as a function of c, and h as a function of u.

For this purpose, in the first place we multiply the first of the equations (30) by $\dfrac{1}{2\sqrt{2}}\,c^{-\frac{n+2}{2}}$, and then, for the sake of abbreviation, denote by a single letter the product which will stand on the left side, putting

$$\xi = \frac{i}{2\sqrt{2}}\,c^{-\frac{n+2}{2}}. \quad . \quad . \quad . \quad . \quad . \quad (31)$$

The equation can then be written in the following form:—

$$\xi = (kc^n)^{-\frac{1}{2}}\left[1 - \frac{n-2}{n}\,\beta\,(kc^n)^{-\frac{1}{n}}\right]. \quad . \quad . \quad . \quad (32)$$

In this equation the quantity ξ is represented as a function of kc^n; and it may be remarked that the second term within the square brackets is very small in comparison with 1, on account of the factor β. This circumstance makes it easy, conversely, to represent kc^n or even its square root as a function of ξ, by forming a series arranged according to ascending powers of β, in which series only a few terms need be taken into account. Thereby is produced the following equation:—

$$k^{\frac{1}{2}}c^{\frac{n}{2}} = \xi^{-1}\left[1 - \frac{n-2}{n}\,\beta\xi^{\frac{2}{n}} - \frac{2(n-2)^2}{n^3}\,\beta^2\xi^{\frac{4}{n}} + \dots\right].$$

If in the last two of the equations (30) we insert the expression for $k^{\frac{1}{2}}$ hence arising, and also the expression which according to (31) holds for i,

$$2\sqrt{2}c^{\frac{n+2}{2}}\xi,$$

we obtain from them the following:—

$$w = c^{-n}\xi^{-2}\left[1 - 2\frac{n-1}{n}\beta\xi^{\frac{2}{n}} + \left(\frac{n-2}{n}\right)^3\beta^2\xi^{\frac{4}{n}} + \ldots\right]; \quad (33)$$

$$h = c^{-n}\xi^{-2}\left[\frac{2}{n}\beta\xi^{\frac{2}{n}} - \frac{2}{n}\left(\frac{n-2}{n}\right)^2\beta^2\xi^{\frac{4}{n}} + \ldots\right]. \quad . \quad . \quad (34)$$

And by subtracting the first of these two equations from the second we get, further,

$$h - w = c^{-n}\xi^{-2}\left[-1 + 2\beta\xi^{\frac{2}{n}} - \left(\frac{n-2}{n}\right)^2\beta^2\xi^{\frac{4}{n}} + \ldots\right]. \quad (35)$$

If, lastly, in this we substitute for ξ again its value given in (31), we obtain $k - w$ represented as a function of i. Here, however, we will not actually carry out this substitution, but, in order to obtain short formulæ, express the result thus:—*If ϕ denotes a function the form of which is determined by the equation*

$$\phi(\xi) = \xi^{-2}\left[-1 + 2\beta\xi^{\frac{2}{n}} - \left(\frac{n-2}{n}\right)^2\beta^2\xi^{\frac{4}{n}} + \ldots\right], \quad (36)$$

then is

$$h - w = c^{-n}\phi\left(\frac{i}{2\sqrt{2}}c^{-\frac{n+2}{2}}\right). \quad . \quad . \quad . \quad (37)$$

At the same time, it follows from (33) that we can put

$$w = \frac{1}{2}c^{-n}\xi\frac{d\phi(\xi)}{d\xi} = \frac{1}{2}c^{-n}i\frac{d\phi\left(\frac{i}{2\sqrt{2}}c^{-\frac{n+2}{2}}\right)}{di}, \quad . \quad . \quad (38)$$

and therefore also

$$w = \frac{1}{2}i\frac{d(h-w)}{di}.$$

It is still easier to represent k as a function of e. For since the signification of e is determined by the equation

$$e = m\overline{\left(\frac{dx}{dt}\right)^2}i = 2mwi,$$

we need only multiply the second of the equations (30) by 2 in

order to obtain

$$e = 4m\sqrt{2k}\left(c - \beta k^{-\frac{1}{n}}\right).$$

If we multiply this by $\dfrac{1}{m4\sqrt{2}}\,c^{\frac{n-2}{2}}$, and represent the product on the left-hand side by a single letter, putting

$$\eta = \frac{e}{m4\sqrt{2}}\,c^{\frac{n-2}{2}}, \quad \ldots \quad \ldots \quad (39)$$

we can give it the form

$$\eta = (kc^n)^{\frac{1}{2}}\left[1 - \beta\,(kc^n)^{-\frac{1}{n}}\right]. \quad \ldots \quad (40)$$

η is here represented as a function of kc^n; and we can proceed with this equation as we did with (32), in order to represent kc^n as a function of η; and thus we get

$$kc^n = \eta^2\left(1 + 2\beta\eta^{-\frac{2}{n}} + \frac{3n-4}{n}\beta^2\eta^{-\frac{4}{n}} + \ldots\right). \quad \ldots \quad (41)$$

We can therefore thus express the result sought:—*If ψ denotes a function the form of which is determined by the equation*

$$\psi(\eta) = \eta^2\left(1 + 2\beta\eta^{-\frac{2}{n}} + \frac{3n-4}{n}\beta^2\eta^{-\frac{4}{n}} + \ldots\right), \quad \ldots \quad (42)$$

then

$$k = c^{-n}\psi\left(\frac{e}{m4\sqrt{2}}\,c^{\frac{n-2}{2}}\right). \quad \ldots \quad \ldots \quad (43)$$

Further, by employing the value found for k, the following value of w can be deduced from equations (30):—

$$w = \frac{1}{2}\,c^{-n}e\,\frac{d\psi\left(\dfrac{e}{m4\sqrt{2}}\,c^{\frac{n-2}{2}}\right)}{de}. \quad \ldots \quad (44)$$

In order, lastly, to exhibit h as a function of u, we multiply the first two of equations (30) into one another, whence arises

$$wi^2 = 8\left(c^2 - 2\frac{n-1}{n}\beta ck^{-\frac{1}{n}} + \frac{n-2}{n}\beta^2k^{-\frac{2}{n}}\right).$$

By putting herein, according to the signification of u,

$$wi^2 = \frac{u}{2m},$$

and then dividing the equation by $8c^2$, we get

$$\frac{u}{16mc^2} = 1 - 2\frac{n-1}{n}\beta\,(kc^n)^{-\frac{1}{n}} + \frac{n-2}{n}\beta^2(kc^n)^{-\frac{2}{n}},$$

which can also be written in the form

$$1-\frac{1}{4c}\sqrt{\frac{u}{m}}=1-\sqrt{1-2\,\frac{n-1}{n}\,\beta\,(kc^n)^{-\frac{1}{n}}+\frac{n-2}{n}\beta^2(kc^n)^{-\frac{2}{n}}}.$$

For the expression on the left-hand side we introduce the symbol ζ, putting

$$\zeta=1-\frac{1}{4c}\sqrt{\frac{u}{m}},\quad . \quad . \quad . \quad . \quad . \quad (45)$$

so that the preceding equation is transformed into

$$\zeta=1-\sqrt{1-2\frac{n-1}{n}\,\beta(kc^n)^{-\frac{1}{n}}+\frac{n-2}{n}\beta^2(kc^n)^{-\frac{2}{n}}}. \quad (46)$$

This equation, in which ζ appears as a function of kc^n, we again make use of in order, conversely, to exhibit kc^n as a function of ζ, remarking that ζ, as is readily seen from the preceding equation, is a small quantity of the order of β, and therefore, in the development of a series according to ascending powers of ζ, the first terms only need to be taken into consideration. We get

$$kc^n=\left(\frac{n-1}{n}\frac{\beta}{\zeta}\right)^n\left[1+\frac{n}{2(n-1)^2}\zeta+\dots\right].\quad . \quad . \quad (47)$$

After attaining this, we divide the last of equations (30) by the first, thereby producing

$$h=\frac{2}{n}\frac{\beta}{c}k^{\frac{n-1}{n}}\left(1+\frac{n-2}{n}\frac{\beta}{c}k^{-\frac{1}{n}}+\dots\right),$$

or, written differently,

$$hc^n=\frac{2}{n}\beta(kc^n)^{\frac{n-1}{n}}\left[1+\frac{n-2}{n}\beta(kc^n)^{-\frac{1}{n}}+\dots\right].$$

Putting in this for kc^n the expression given in (47), we get

$$hc^n=2\left(\frac{\beta}{n}\right)^n\left(\frac{n-1}{\zeta}\right)^{n-1}\left[1+\frac{2n-3}{2(n-1)}\zeta+\dots\right].\quad . \quad (48)$$

We can therefore express the sought-for result in the following manner :—*If χ is a function the form of which is determined by the equation*

$$\chi(\zeta)=2\left(\frac{\beta}{n}\right)^n\left(\frac{n-1}{\zeta}\right)^{n-1}\left[1+\frac{2n-3}{2(n-1)}\zeta+\dots\right],\quad . \quad (49)$$

then is

$$h=c^{-n}\chi\left(1-\frac{1}{4c}\sqrt{\frac{u}{m}}\right).\quad . \quad . \quad . \quad . \quad (50)$$

Simultaneously, by the employment of (47), we obtain from

equations (30) :—

$$w = c^{-n}u \frac{d\chi\left(1 - \frac{1}{4c}\sqrt{\frac{\bar{u}}{m}}\right)}{du}. \quad . \quad . \quad . \quad (51)$$

The hitherto deduced expressions of $h - w$, k, and h refer to only one coordinate-direction. But of course equations of precisely corresponding form are valid for all three directions—which equations are obtained when in equations (37), (43), and (50) first the index 1 is annexed to each of the letters h, w, k, i, e, u, and c, then the index 2, and finally the index 3. Remembering further that we may put

$$\bar{U} = m(h_1 + h_2 + h_3),$$
$$\bar{T} = m(w_1 + w_2 + w_3),$$
$$E = m(k_1 + k_2 + k_3),$$

we get

$$\bar{U} - \bar{T} = m\left[c_1^{-n}\phi\left(\frac{i_1}{2\sqrt{2}}c_1^{-\frac{n+2}{2}}\right) + c_2^{-n}\phi\left(\frac{i_2}{2\sqrt{2}}c_2^{-\frac{n+2}{2}}\right)\right.$$
$$\left. + c_3^{-n}\phi\left(\frac{i_3}{2\sqrt{2}}c_3^{-\frac{n+2}{2}}\right)\right],$$

and corresponding equations for E and $\bar{U}$. If by the sign of summation we abbreviate the three terms on the right-hand side in each, the three equations become

$$\bar{U} - \bar{T} = m\Sigma c^{-n}\phi\left(\frac{i}{2\sqrt{2}}c^{-\frac{n+2}{2}}\right),$$
$$E = m\Sigma c^{-n}\psi\left(\frac{e}{m4\sqrt{2}}c^{\frac{n-2}{2}}\right), \quad \left.\right\} \quad . \quad . \quad . \quad (52)$$
$$\bar{U} = m\Sigma c^{-n}\chi\left(1 - \frac{1}{4c}\sqrt{\frac{\bar{u}}{m}}\right),$$

wherein ϕ, ψ, and χ are the functions above determined by equations (36), (42), and (49).

§ 7. In the equations of the preceding section, the quantities i_1, i_2, i_3 have been employed for the determination of $\bar{U} - \bar{T}$, for the determination of E the quantities e_1, e_2, e_3, and to determine $\bar{U}$ the quantities u_1, u_2, u_3. For many investigations, however, it is advantageous to effect these determinations by means of one and the same system of quantities, to which purpose the quantities w_1, w_2, w_3 are particularly adapted. Of course, as in all the previous equations, together with them occur also the constants c_1, c_2, c_3.

When the second of the equations (30) is divided by the first,

the result is

$$w = k \; \frac{c - \beta k^{-\frac{1}{n}}}{c - \dfrac{n-2}{n} \beta k^{-\frac{1}{n}}}.$$

From this equation another can easily be derived, in which k is presented as a function of w; and if we at the same time take into consideration that $h = k - w$, we obtain :—

$$\left.\begin{aligned}
k &= w + \frac{2}{n} \frac{\beta}{c} w^{\frac{n-1}{n}} + 2 \frac{n^2 - 2}{n^3} \frac{\beta^2}{c^2} w^{\frac{n-2}{n}} + \ldots, \\[2mm]
h &= \frac{2}{n} \frac{\beta}{c} w^{\frac{n-1}{n}} + 2 \frac{n^2 - 2}{n^3} \frac{\beta^2}{c^2} w^{\frac{n-2}{n}} + \ldots, \\[2mm]
h - w &= -w + \frac{2}{n} \frac{\beta}{c} w^{\frac{n-1}{n}} + 2 \frac{n^2 - 2}{n^3} \frac{\beta^2}{c^2} w^{\frac{n-2}{n}} + \ldots.
\end{aligned}\right\} \quad (53)$$

We will in like manner express also the three quantities belonging to k, h, and $h - w$, viz. e, u, and i, or, still better, their logarithms, as functions of w. In order, first, to determine i, we employ the first of the equations (30), and put therein for k the expression given in the first of the equations (53), by which we get

$$i = 2\sqrt{2} \, c w^{-\frac{1}{2}} \left[1 - \frac{n-1}{n} \frac{\beta}{c} w^{-\frac{1}{n}} + \frac{3n-4}{2n^3} \frac{\beta^2}{c^2} w^{-\frac{2}{n}} + \ldots \right].$$

If from this we derive the equation for $\log i$, considering that we can put

$$e = 2mwi \quad \text{and} \quad u = 2mwi^2,$$

and accordingly

$$\log e = \log i + \log 2mw \quad \text{and} \quad \log u = 2 \log i + \log 2mw,$$

we get

$$\left.\begin{aligned}
\log i &= \log \left(2\sqrt{2} \, c w^{-\frac{1}{2}} \right) - \frac{n-1}{n} \frac{\beta}{c} w^{-\frac{1}{n}} \\[1mm]
&\quad - \frac{(n^2 - 2)(n-2)}{2n^3} \frac{\beta^2}{c^2} w^{-\frac{2}{n}} + \ldots \\[2mm]
\log e &= \log \left(4\sqrt{2} \, mc w^{\frac{1}{2}} \right) - \frac{n-1}{n} \frac{\beta}{c} w^{-\frac{1}{n}} \\[1mm]
&\quad - \frac{(n^2 - 2)(n-2)}{2n^3} \frac{\beta^2}{c^2} w^{-\frac{2}{n}} + \ldots \\[2mm]
\log u &= \log \left(16 mc^2 \right) - 2 \frac{n-1}{n} \frac{\beta}{c} w^{-\frac{1}{n}} \\[1mm]
&\quad - \frac{(n^2 - 2)(n-2)}{n^3} \frac{\beta^2}{c^2} w^{-\frac{2}{n}} + \ldots.
\end{aligned}\right\} \quad \cdot \; \cdot \quad (54)$$

§ 8. In order now to form those differential coefficients of k, h, and $h-w$ with which we have to do in the theorem of the mean ergal (of which we select h for consideration first), we must inquire in what relation the two partial differential coefficients obtained when h is represented as a function u and c stand to those which are obtained when h is exhibited as a function of w and c. I wish, however, to premise a few remarks on the notation.

In cases like the present, where a quantity is represented as a function of two variables, while, from the nature of the subject, we do not always employ the same two, but sometimes change the variables, and where, consequently, partial differential coefficients occur which only differ from one another by the fact that the quantity which in the differentiation was presupposed constant is different in one of them from what it is in another, it is convenient to indicate this in the formula, in order that it may not be necessary always to state it verbally. I have therefore, in a previous memoir*, used a notation which is also adopted by various other authors in treating the same subject; that is, I have added as an index the quantity which in the differentiation is regarded as constant. The external form, however, in which this was done can be simplified if, instead of putting the index with the entire differential coefficient (in which case the latter must be enclosed in brackets, and also confusion may arise with other indices possibly occurring in this place, unless the index be furnished with a distinguishing mark), we put it with the d in its numerator; and this is the form in which we will here employ that notation.

If, for example, the quantity h is regarded, first, as a function of u and c and so differentiated according to c (when u is considered a constant), and, second, as a function of w and c and so differentiated according to c (when w is considered a constant), we write the two differential coefficients thus,

$$\frac{d_u h}{dc} \text{ and } \frac{d_w h}{dc}.$$

This way of writing them agrees with that which I employed, in the memoir "On a new Mechanical Theorem relative to Stationary Motions"†, for the specializing of variations, as I put the *measuring* quantity (which is regarded in the variation as constant) as an index to the δ‡.

* Pogg. *Ann.* vol. cxxv. p. 368; *Abhandlungensammlung,* vol. ii. p. 14.

† *Sitzungsb. d. Niederrhein. Ges. für Natur- und Heilkunde,* 1873; Phil. Mag. S. 4. vol. xlvi. pp. 236, 266.

‡ Just as my memoir was ready for the press, I received the just published new Part of the *Fortschritte der Physik,* and observed in it that Boltzmann also has simplified the form of the index-furnished differential

Having made these preliminary remarks, we will form the differential coefficients which are to be compared. For this purpose we first consider h as a function of u and c, and form the differential equation

$$dh = \frac{d_c h}{du} du + \frac{d_u h}{dc} dc.$$

Herein let us now imagine u represented as a function of w and c; we can then write the equation thus :—

$$dh = \frac{d_c h}{du}\left(\frac{d_c u}{dw} dw + \frac{d_w u}{dc} dc\right) + \frac{d_u h}{dc} dc$$

$$= \frac{d_c h}{du}\frac{d_c u}{dw} dw + \left(\frac{d_c h}{du}\frac{d_w u}{dc} + \frac{d_u h}{dc}\right) dc.$$

If, on the other hand, we regard h as a function of w and c, we can write

$$dh = \frac{d_c h}{dw} dw + \frac{d_w h}{dc} dc.$$

Now, as in this and the preceding equation the coefficients of dw, and in like manner the coefficients of dc, must be equal to one another, we get

$$\frac{d_c h}{du}\frac{d_c u}{dw} = \frac{d_c h}{dw},$$

$$\frac{d_c h}{du}\frac{d_w u}{dc} + \frac{d_u h}{dc} = \frac{d_w h}{dc},$$

or by transposition

$$\left.\begin{aligned} \frac{d_c h}{du} &= \frac{\dfrac{d_c h}{dw}}{\dfrac{d_c u}{dw}}, \\[2em] \frac{d_u h}{dc} &= \frac{d_w h}{dc} - \frac{d_c h}{du}\frac{d_w u}{dc}. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (55)$$

We will give these equations a form somewhat different and more adapted for our use, putting

---

coefficient; for in his " Berichte " (p. 441) he denotes a differentiation according to x in which y is considered constant by $\frac{d}{dx_y}$. Although this notation is convenient, yet I think the one I have chosen may be retained, because the index is not intended to specialize the variable in the denominator, but the nature of the differentiation, and therefore, in my opinion, is better placed close to the d (which denotes the differentation) of the numerator.

$$\frac{d_c h}{du} = \frac{1}{u} \frac{d_c h}{\frac{1}{u} du} = \frac{1}{u} \frac{d_c h}{d \log u},$$

$$\frac{d_c u}{dw} = u \frac{d_c \log u}{dw} \text{ and } \frac{d_w u}{dc} = u \frac{d_w \log u}{dc},$$

by which they are transformed into

$$\left.\begin{array}{l} \dfrac{d_c h}{d \log u} = \dfrac{\dfrac{d_c h}{dw}}{\dfrac{d_c \log u}{dw}}, \\[3mm] \dfrac{d_u h}{dc} = \dfrac{d_w h}{dc} - \dfrac{d_c h}{d \log u} \dfrac{d_w \log u}{dc}. \end{array}\right\} \quad . \quad . \quad (56)$$

If, in the first of these two equations, instead of h and $\log u$ we employ the expressions given in (53) and (54), we obtain

$$\frac{d_c h}{d \log u} = w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

(as it must be, according to the theorem of the mean ergal); and accordingly the second of the two preceding equations can be written in the following simplified form :—

$$\frac{d_u h}{dc} = \frac{d_w h}{dc} - w \frac{d_w \log u}{dc}. \quad . \quad . \quad . \quad . \quad (58)$$

In an entirely corresponding manner we get :—

$$\left.\begin{array}{l} \dfrac{d_c k}{d \log e} = 2w, \\[3mm] \dfrac{d_e k}{dc} = \dfrac{d_w k}{dc} - 2w \dfrac{d_w \log e}{dc}, \end{array}\right\} \quad . \quad . \quad . \quad (59)$$

$$\left.\begin{array}{l} \dfrac{d_c (h-w)}{d \log i} = 2w, \\[3mm] \dfrac{d_i (h-w)}{dc} = \dfrac{d_w (h-w)}{dc} - 2w \dfrac{d_w \log i}{dc}. \end{array}\right\} \quad . \quad (60)$$

Equations (53) refer to only one coordinate-direction; but from them the corresponding equations for all three directions can be forthwith derived, which determine the quantities $\overline{U}$, E, and $\overline{U} - \overline{T}$ as functions of w_1, w_2, w_3, c_1, c_2, c_3. In order that the formulæ we obtain may not be too long, we will first give to the second of the equations (53) the following

form,

$$h = c^{-n} \left[\frac{2}{n} \beta (wc^n)^{\frac{n-1}{n}} + 2 \frac{n^2-2}{n^3} \beta^3 (wc^n)^{\frac{n-2}{n}} + \dots \right],$$

and express the meaning of this equation as follows :—*If* f *signifies a function whose form is determined by the equation*

$$f(\sigma) = \frac{2}{n} \beta \sigma^{\frac{n-1}{n}} + 2 \frac{n^2-2}{n^3} \beta^3 \sigma^{\frac{n-2}{n}} + \dots, \quad . \quad . \quad (61)$$

then is

$$h = c^{-n} f(wc^n). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

This latter we can forthwith extend in such a way as to determine the quantity $\overline{U} = m(h_1 + h_2 + h_3)$, viz.

$$\overline{U} = m \left[c_1^{-n} f(w_1 c_1^n) + c_2^{-n} f(w_2 c_2^n) + c_3^{-n} f(w_3 c_3^n) \right],$$

or, employing the sign of summation,

$$\overline{U} = m \Sigma c^{-n} f(wc^n). \quad . \quad . \quad . \quad . \quad . \quad (63)$$

In order to derive from this expression of $\overline{U}$ the expressions of the two other quantities requiring determination, we need only put

$$E = \overline{U} + \overline{T} = \overline{U} + m\Sigma w, \quad . \quad . \quad . \quad . \quad (64)$$

$$\overline{U} - \overline{T} = \overline{U} - m\Sigma w. \quad . \quad . \quad . \quad . \quad . \quad (65)$$

[To be continued.]

VII. *On Photographic Irradiation.*
By Captain ABNEY, *R.E., F.R.A.S., F.C.S.*[*]

THE most frequently received notion regarding photographic irradiation (*i. e.* an increase on development of the apparent size of the image of a luminous object when photographed against a darker body) is, that it is due to simple reflection of the incident rays from the back of the plate. This view is quite untenable, excepting in the case where the incident rays fall at an angle with the perpendicular to the surface of the plate. If a bromo-iodized film be sensitized in an ordinary nitrate-of-silver bath and be examined with a microscope, using a high power, it will be found that it consists of particles of bromo-iodide of silver separated by considerable intervals one from the other. The collodion vehicle is illuminated with that peculiar greenish-yellow tint which always marks a film of this description. A few simple experiments will show that this colour is entirely due to the light reflected from the particles, and that it is not due to the collodion itself.

* Communicated by the Author.

When the construction of the sensitive film is taken into account, the cause of irradiation (or blurring of the image) is not far to seek.

It is these small particles that cause the film to be translucent and prevent its being transparent. Where there is transparency there must be a scattering of the incident rays.

These particles are quite measurable; hence we cannot treat the question as if they were small fractions of a wave-length. In fact they are several times larger in diameter than the greatest wave-length; and as we have to deal with the smaller wave-lengths, we may treat the question as a case of simple geometrical reflection. At any rate, the dispersion caused would not interfere with the general result.

The physical forms which the particles take are not quite apparent. Being in an amorphous state, it will not be unfair to assume that they are generally of the shape of a spheroid. For the sake of calculation, we may also assume that each particle reflects a definite proportion of light. This hypothesis will be found not to affect the aspect of the problem, if the refracted rays be traced after transmission through the sphere, in a similar manner to that in which it is proposed to trace the reflected rays. In any case it is only assumed that an approximate solution can be obtained.

Let us consider the action of one particle, and, for the sake of simplicity, let us take it to be one in contact with the surface of the glass plate on which the film rests. The case where the minimum irradiation will be produced is that in which the incident ray falls perpendicularly on the surface. First, on these hypotheses let us examine the intensity and duration of the light that would fall on the plate.

As the general direction of the light is perpendicular to the surface, it is evident that by far the largest proportion of it must fall in the same direction on the particle under consideration.

Fig. 1.
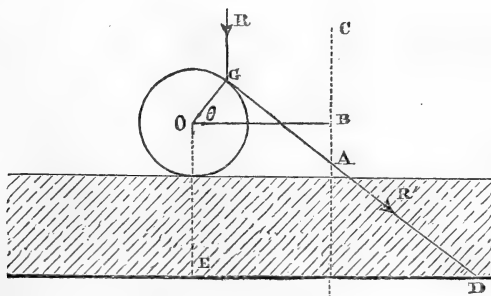
Let $r =$ the radius of the particle.

   $\theta =$ the angle of incidence of R, a ray of the pencil falling on it.

Let A E represent a section of the glass plate.

Let* E D $= x$, and $t =$ thickness of the plate.

Since E G D $= 2\theta$,

$$\therefore \quad x = r \cos \theta + (t + r + r \sin \theta) \tan 2\theta.$$

From the question before us it is manifest that θ cannot be greater than $\dfrac{\pi}{4}$.

When r is small in comparison with t,

$$x = t \cdot \tan 2\theta,$$

$$\frac{dx}{d\theta} = \frac{2t}{\cos^2 2\theta}.$$

Now the quantity of light falling upon a very small surface at G will be distributed on a small surface about D .

The small surface at G may be represented by

$$r^2 \sin \theta \cos \theta \cdot \delta\theta \cdot \delta\phi$$

(where ϕ is the angular measure taken parallel to the surface of the plate).

The small surface at G will be

$$x\delta x \cdot \delta\phi = t \cdot \tan 2\theta \times \frac{2t}{\cos^2 2\theta} \delta\theta \cdot \delta\phi.$$

If I and I′ be respectively the intensities of the light reflected from G and falling on D,

$$\mathrm{I'} = \frac{\mathrm{I}r^2 \sin \theta \cos \theta}{2t^2 \dfrac{\tan 2\theta}{\cos^2 \theta}} = \frac{\mathrm{I}r^2}{4t^2} \cos^3 2\theta.$$

Since $x = t \cdot \tan 2\theta$,

$$\mathrm{I} = \frac{\mathrm{I}r^2 t}{4} \cdot \frac{1}{(t^2 + x^2)^{\frac{3}{2}}}.$$

For the sake of illustration, the accompanying curve has been constructed showing the general relation between the intensity and angle of incidence on the bottom surface of the plate.   The ordinates represent the intensities.   For the abscissæ the thickness of the plate has been taken as the unit of measure.

The solid of revolution formed by the area rotating round the axis of y would give the projection of the pencil of rays incident on the particle multiplied by its intensity.   Not all these rays will be reflected back ; only those which fall at and

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* The refraction-index of collodion and the glass may be taken as equal.

beyond the visual angle will do so. Calculating according to Fresnel's formulæ the amount of light that would be reflected

Fig. 2.
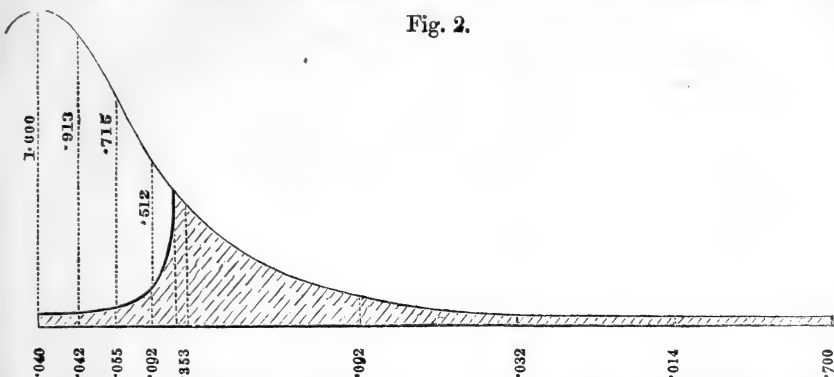
for each angle less than the critical angle, we get the curve which bounds the shaded part of the figure. The area between the two curves shows the amount that would pass through. The refractive index has been taken as 1·5.

An examination of the figure shows that if a ray of light fall on the film, on development a ring *might* be formed round the image at a distance from it equal to $2t \cot \chi$, where χ is the critical angle; on the inside it would be shaded off more abruptly than on the outside. If then a cylindrical pencil of rays strike a film perpendicular to its surface, a similar detached annulus would result, provided the diameter were $< 2t \cot \chi$.

To ascertain if actual results held with the theory, the following experiments were made :—

(i) A short-focus lens (about 6 inches) was employed to form an image of the sun on a sensitive plate whose thickness was approximately ·12 inch. On developing the image a well-defined annulus was obtained, the most intense portion being equal to the sun's diameter. The shading-off was that described above, and answered to the theory.

(ii) A plate of double the thickness of the foregoing was similarly treated. The mean diameter of this annulus was double that of the last.

(iii) Two plates, similar to that used in (i), were next experimented with. Each one carried a sensitive film. The film of the bottom plate, 1 (fig. 3), was separated from the surface of the top plate, 2, by a strip of thin card to prevent contact. On developing them the ring was obtained on the first plate, and a diffused image on the second.

Throwing the two images on a screen, and taking the inten-

sities by the method described in my paper in the Philosophical Magazine for September 1874, I obtained two curves which, when

Fig. 3.
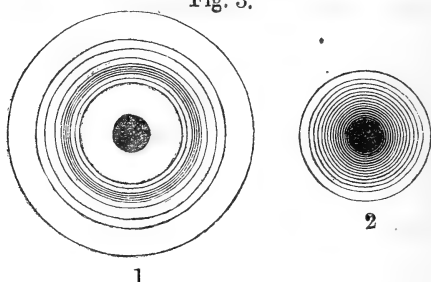
the sun's diameter was taken into account, as also the irradiation due to the cause which I shall presently explain, gave a near coincidence to that obtained by this theory.   It should be noted that the image obtained on the bottom film was that due to the light refracted and not reflected.   In the films employed, about $\frac{1}{20}$ of the actinic rays were transmitted through the first plate, and the amount of reflection from the sensitive film itself was nearly $\frac{1}{5}$ of the whole light.

(iv) With dry plates I also made other experiments.   I cut small holes and slits in platinum foil.   Placing these in contact with the plate, it was exposed to direct sunlight.   With the former, the rings were obtained as before; with the latter, parallel lines joined with semicircles.   This gave sufficient evidence that nothing in the lens used in (i), (ii), and (iii) could cause the effects produced.

If light be scattered from particles, it must also take place in a direction parallel to the surface of the film—that is, in the film itself.   Since the thickness of the film is so small, we need only consider the amount that would fall on a plane at right angles to the surface.   With the same hypotheses as before, and treating it similarly, and taking B as the origin of x along BC and OB$=h$, we have

$$x = r \sin \theta + (h - r \cos \theta) \cot 2\theta.$$

If we make h large compared with r,

$$x = h \cot 2\theta ;$$

as before we obtain

$$I' = \frac{I r^2 h}{4} \cdot \frac{1}{(h^2 + x^2)^{\frac{3}{2}}},$$

and the same curve obtains as in the last case, showing that the greatest intensity of light is in the direction parallel to the surface of the film.

When h is not large compared with r, the same result holds,

though the plane of maximum reflection will not pass through the centre of the particle. The light reflected horizontally would obey the ordinary laws of absorption, and the intensity be represented by

$$I' = I\epsilon^{-\mu z},$$

where z is the distance along the film measured from the point of incidence.

If the relative effect on the sensitive film could be represented by IT, where T is the length of exposure, then it would be easy to calculate exactly the distance to which the irradiation from this cause would extend. Experiment has shown that this simple relation does not hold good, the practical results having been given in the above-quoted paper. There it will be seen that the shorter the exposure the less proportional irradiation there must be. If, however, we take this relation to hold good, the effects of irradiation in any example we may work will be in excess of the truth. In the experiment (i) it was found that the limit of irradiation by reflection with $\frac{1}{4}$ second's exposure was caused by an intensity of sunlight $= \cdot 0016$, together with that caused by the light from the sky. The sky-light would also equally act on that part of the film which would be affected by the light scattered parallel to the plate. The thickness of the film was $\cdot 004$ inch; hence $\mu = 750$ nearly for each inch.

From other experiments taking images on opaque surfaces, it was found that, with similar exposure and under similar conditions, the extent of developable irradiation was nearly $\cdot 005$.

By taking the mean intensity of the light passing through the film, we can find approximately the proportion of light which is scattered parallel to surfaces to that reflected from the plate. Let m be ratio,

$$\cdot 0016 = \cdot 3\epsilon^{-750 \times \cdot 005} \times m \, ;$$
$$m = \cdot 23.$$

If the sun's image were photographed (say) 4 inches in diameter with an effective aperture of 2 inches, instead of $\cdot 05$ and $1\frac{1}{2}$ inch respectively, it would be seen, calculating from the above data, that with an exposure of $\frac{1}{100}''$ (supposing the simple relation between I and T held good) no developable image could be formed, much less could there be irradiation due to either cause. Now it is well known that with the photoheliograph under these conditions a good image of a clear sun is obtainable. Evidently, then, the connexion between I and T is of a more complicated nature.

It will be noticed that a large difference in two intensities of light (giving the same exposure on the same film) will make but a small difference in the extent of irradiation, as the coeffi-

cient of absorption is so large. In the case of the late Transit of Venus this has an important bearing, as the actinic intensity of the sun diminishes from the centre to the limb, the highest estimate given for it being from about 1 to $\frac{1}{14}$. The increase in diameter of the sun and decrease in that of Venus will be measurably the same.

To diminish irradiation due to the first cause pointed out, three methods are open:—first, to coat the back of the plate with some non-actinic colour; secondly, to increase the opacity of the film; thirdly, to connect the particles of bromo-iodide of silver with a body of as nearly as possible the same density. The first is the usual method adopted by photographers in dry-plate processes. This *diminishes* the irradiation from the back of the plate, but does not completely eliminate it, as will be understood when the nature of the resulting reflection is considered. The second method is easily carried out, and is more effective than the first method. The last is a plan adopted in certain dry-plate processes. With albumen preservative particularly, the amount of irradiation is reduced materially, even though the film appears nearly transparent. One other method may be noticed, viz. staining the collodion itself with an adiactinic colour. This does not seem to commend itself, as the sensitiveness of the plate must be reduced in proportion to its efficacy.

To diminish irradiation due to the second cause, Nos. 1, 2, & 3 methods are effective.

Since writing this paper I find that my friend Mr. Cowper Ranyard has noticed, though I believe has not published, the fact that irradiation is most marked at a distance corresponding to the critical angle of reflection in glass.

VIII. *On an Apparatus to illustrate the Formation of Volcanic Cones.* By C. J. WOODWARD, B.Sc.[*]

A SHORT time ago I spent some weeks in Central France, and had many opportunities of studying the character of the volcanic cones which are so abundant in that district. The many sections of these cones that may be seen at Graveneire and other places, where the scoriæ is obtained for industrial purposes, show their structure clearly, and one can realize the truth and simplicity of the theory accounting for their formation. The theory is so obviously correct that it has not, so far as I know, been subjected to experiment on a small scale. The object of this paper is to describe an apparatus that may be used in the lecture-room to build up model cones and craters.

Soon after my return I tried an experiment at some Iron-works to make such a cone. A large sheet of iron with a hole in the centre was placed over a pipe proceeding from the blast. A quantity of cinders and ashes were supplied to the pipe, and in a short time a cone was built up. For the purpose of lecture-illustration the apparatus about to be described is convenient, and shows very clearly the formation of volcanic cones. It consists of a wooden trough about 18 inches long, with sloping sides; at the bottom of the trough there is a bladed screw to carry forward the ashes, sawdust, or other material used, to an opening through which air from a powerful bellows is forced upward in a vertical stream. A board 3 or 4 feet square with a hole in the centre is placed over the air-jet, so that on turning the screw the sawdust is carried into the stream of air; it is then thrown vertically upward through the hole in the upper board, and on falling down builds up a cone having many of the peculiarities of a natural one. Thus, the angle of slope is almost identical; the sorting of materials is similar, the heaviest portions being near the crater, while the finer particles and dust are carried to a great distance. If, while the miniature eruption is going on, the oxyhydrogen light be directed horizontally on to the cone, it will be seen that the dust is carried to a distance of many feet. Using alternately sawdust of different colours, the structure of the cone is well seen on making a section of it. This is easily done by cutting vertically through the cone with a sheet of cardboard and sweeping off one half of the mound into a tray. If the jet of air be vertical and the air of the room is still, the lip of the crater is horizontal; but if the apparatus be near the door or where there is a draught, the materials are carried to one side, and the cone is built up much higher on one side than the other. The same thing, of course, will occur if the jet is not vertical; and as from accidental circumstances the jet frequently changes its direction, a considerable variety of cones is sure to be obtained in a series of experiments.

Birmingham, May 15, 1875.

IX. *On Friction and Heat-conduction in rarefied Gases.*
By A. KUNDT *and* E. WARBURG[*].

OUR knowledge of the properties of gaseous bodies has of late years been considerably extended by the consequences deduced from the kinetic molecular theory of these bodies established by Clausius. The remarkable laws theoretically disco-

vered by Maxwell, in regard to the friction, conduction of heat, and diffusion of gases, have been confirmed by the experiments of Maxwell, O. E. Meyer, Loschmidt, Stefan.   Only with respect to the absolute value of the coefficient of heat-conduction of air as calculated by Maxwell does a deviation appear, according to Boltzmann; but the law that this coefficient is independent of the pressure has been experimentally proved by Stefan between $\frac{1}{2}$ and 1 atmosphere.   The confidence excited by these results has induced us to pursue the theory to further consequences.

As is well known, so far as it has yet been worked out, this theory necessarily presupposes that the so-called mean length of path of the molecules is a quantity that may be neglected against the linear dimensions of the space filled with the gas.   But as the mean length of path is inversely proportional to the density, that is equivalent to saying that the density of the gas in a given space must not be too little.   We do not know that the theory has yet been developed for cases in which the above supposition falls; the experiments have always been so arranged that it holds good.   The aim of our investigation (of which we take leave to lay the following brief abstract of the results before the Academy) is to trace the properties of gases beyond the limits of density mentioned.

Friction.

Let us first consider the following simple case.   A layer of air of the thickness d is between two solid plane partitions, of which the one is at rest, while the other moves with the velocity u'_0. When the gas is more and more rarefied, the mean length of path l more and more increases; an easy calculation shows that the gas of the layer has still sensibly the same properties in regard to friction as the same gas in thicker layers, so long as $\frac{l}{d} < \frac{1}{14}$.   Up to that point the retarding force exerted upon the moved partition will be, to a thousandth part of its value, constant, and, as calculation shows, rapidly diminish when the corresponding degree of rarefaction is exceeded.

We are here struck by an apparent contradiction to this result of the theory; for we found that even when $\frac{l}{d} = \frac{1}{300}$, a perceptible diminution of the retarding forces commences, becoming greater as the pressure diminishes.   The contradiction, however, is only apparent; the phenomena observed may be deduced from a proposition which we have derived from the theory. viz. that there is sliding of a gas on a solid partition, and that the coefficient of sliding is inversely proportional to the pressure.

Indeed the velocity u_0 of the layer of gas adjacent to the moved

partition is equal to the mean velocity of translation of the mole-
cules passing through it in both directions. But one half of
these come from the partition, and have consequently a mean
velocity of translation $\overline{\overline{\lessgtr}} u'_0$; the other half come from the inte-
rior of the gas, and therefore have a mean velocity of translation
$< u'_0$. Consequently $u_0 < u'_0$.

A closer investigation, to be given in detail in another place,
has led to the following results.

1. The sliding-coefficient for a gas and a solid partition has
sensibly a determined value dependent on the nature of the gas,
so long as the latter is present in layers thicker than fourteen
times the mean length of path; and it is inversely proportional
to the pressure.

2. The absolute value of the sliding-coefficient is obtained by
admitting that the gas-molecules are reflected from the partition
with its velocity of translation, to $1 \cdot 4 \dfrac{l}{2}$,—consequently for air,
for which at 760 millims. pressure $l = 0 \cdot 000083$ millim. accord-
ing to Stefan, to $0 \cdot 000058 \dfrac{760}{p}$, p being the pressure in millims.
of mercury.

From our experiments it comes to about twice as much—
namely, in round numbers, to $0 \cdot 0001 \dfrac{760}{p}$ millim. From this
we may conclude that on the collision of the molecules with the
partition the velocities of translation of the two are not perfectly
balanced. By means of this value, it is easy to satisfy ourselves
that in Maxwell and Meyer's experiments the effect of the sliding
cannot have become sensible.

Our experiments on friction were conducted according to
Maxwell's method; for we measured the logarithmic decrement
of the torsional vibrations executed by a disk between two fixed
disks sufficiently near to it. As we made the weight of the glass
disk (diameter 159 millims., weight 61·9 grammes) so small
that it was borne by two very thin silver wires (diam. = 0·0630
millim.) in a bifilar suspension, we succeeded in making the dis-
turbing damping moments so slight that they might be neglected:
with the smallest decrements measured they only amounted to
a little over 1 per cent., and with nearly all the measurements
employed for calculations much less than 1 per cent., of the total
value observed. We thus obtained a substantial simplification
of the apparatus, *one* vibrating disk being sufficient. We will
here put, first, some measurements effected by means of our ap-
paratus, at pressures of 750 and 380 millims.

I. Absolute coefficient of friction μ of air, determined from three series of experiments.

Thickness of the stratum of air. μ for 15° Cels.

gr.

$d_1 = 0.1104$ centim.   0.000193 centim. sec.
$d_2 = 0.1967$ „   186   „
$d_3 = 0.2802$ „   189   „

                    Mean . 0.000189   „

We attach the least importance to the value obtained from d_1, since the distance of d_1 was determined less accurately than those of d_2 and d_3.

Maxwell finds for 15° C. 0.000198
Meyer, from his last experiments by Maxwell's $\big\}$ 0.000197
    method . . . .
Meyer, from transpiration experiments . . . 0.000182
Puluj*, from transpiration experiments . . . 0.000185

Our value therefore approaches nearer than the older ones to that deduced from transpiration experiments.

II. Putting μ for air $= 1$, the following values of this quantity were obtained :—

For	From experiments with the distances			Mean.	From Graham's transpiration experiments.
	d_1.	d_2.	d_3.		
Hydrogen	0.495	0.485	0.484	0.488	0.4855
Carbonic acid ...	0.811	0.805	0.801	0.806	0.807

Maxwell explains the divergent results obtained by him for hydrogen and carbonic acid by impurity of the gases employed.

III. The coefficient of friction of pure aqueous vapour for 15° Cels., measured at 21° Cels., and with a tension of about 16 millims., amounted to 0.526 of that of air at 15°.

Passing now to the experiments which were made for testing the above theory, we remark that the chief difficulty in them consisted in placing in a given space gases of small density as pure as possible. It was soon found that here nothing could be effected with caoutchouc connexions; and hence all the junctions used were made of glass; while, in order to secure the apparatus against fracture by expansion of the glass, at suitable places thick, wide, elastic, glass tubes bent in three directions at right angles to one another were inserted, which gave to the

* *Sitzungsberichte der Wiener Akademie*, 1874, vol. lxix. p. 289.

whole the requisite flexibility. Exhaustion was produced by means of a Geissler mercury air-pump.

The following Tables show the dependence of the logarithmic decrement, λ, on the pressure, p. All the numbers are reduced to 15° Cels. (the mean temperature of the experiments) ; and, following Maxwell, we have supposed the friction-coefficient proportional to the absolute temperature.

Air.					
	centim. $d_1 = 0.1104$		centim. $d_2 = 0.1967$		centim. $d_3 = 0.2802$
p.	λ.	p.	λ.	p.	λ.
millim. 380	0.132	millim. 750–380	0.0583	millim. 750–370	0.0425
20	0.131	20.5	0.0582	20.5	0.0424
7.6	0.129			7.3	0.0419
2.4	0.125	2.4	0.0567	2.4	0.0413
1.53	0.120	1.53	0.0554	1.53	0.0405
Hydrogen.		Carbonic Acid.			
380	0.0652	750–380	0.0469	750–380	0.0341
20	0.0638	20.5	0.0467		
8.8	0.0629			8.8	0.0338
2.4	0.0601	2.4	0.0461	2.4	0.0336
1.53	0.0557	1.54	0.0453	1.48	0.0331

A glance at these Tables shows, first, that, in accordance with the theory of sliding, the less the thickness of the layer of air engaged in friction, the greater is the decrease per cent. of the logarithmic decrement with the pressure for a given gas. Further, numerically, we should have

$$\lambda = \frac{\lambda_0}{1 + \dfrac{a}{p}} = \frac{\lambda_0}{1 + \delta},$$

if λ_0 is the value of λ for $\dfrac{d}{l} = \infty$.

δ should be (1) inversely proportional to the pressure ; (2) inversely proportional to the thickness d of the stratum of air in friction ; (3) for different gases, directly proportional to the mean length of path with the pressure of an atmosphere.

To test these laws, we calculated one set of series of experiments from the others ; and found differences only rarely reaching 1 per cent. of the total observed value. Certainly the constancy of the value of $a \cdot d$ for one gas, and consequently the

correctness of the theory, cannot be strictly demonstrated from the experiments, since $\frac{a}{p}$ is everywhere too small in proportion to 1 ; but if the theory be assumed, the constancy of the friction-index μ down to 1·5 millim. pressure is proved by these experiments.

On proceeding, further, to investigate the laws of gas-friction *below* the before-mentioned limit of rarefaction $\left(\frac{l}{d} > \frac{1}{14}\right)$, we could not succeed, even with the most careful drying, in removing with sufficient completeness the last traces of aqueous vapour, which, insensible in the above experiments, with the *low* pressures here employed distorted the results. The presence of aqueous vapour was shown *inter alia* by this—that the damping-moment for a vacuum (so we name a space filled with a gas of $\frac{1}{100}$ of a millim. pressure mixed with vapour) rose considerably when the apparatus was left to itself. This arose from water separating from the solid parts and evaporating into the vacuum. In consequence of this, the theory cannot be quantitatively tested on the results obtained. Nevertheless the following series of experiments shows how far we were able to reduce the logarithmic decrement λ by rarefying the gas. The thickness of the friction-stratum amounted to 1 millim.

Hydrogen.

	p.	λ.
June 30.	760 millims.	0·0387
	Vac. I.	0·0180
	Vac. II.	0·0140
	Vac. III.	0·0119
July 1.	,,	0·0220

Vacua II. and III. were obtained by causing the little gas-bubble to pass out of the receiver of the mercury air-pump into a vacuum, until at last, with vacuum III., nothing more passed out, even into that. Hence vacuum III. is probably to be regarded as an aqueous-vapour vacuum, in which the mean length of path is much greater than $\frac{1}{14} d$. Through the taking away of very slight traces of hydrogen (vac. I.–vac. III.) the logarithmic decrement sinks to $\frac{2}{3}$ of its value. From this we see how, in accordance with the gas-theory, proportionally large quantities of magnitude of motion can be conveyed in the unit of time by traces of gaseous matter.

Heat-conduction.

If we contemplate the simplest case of heat-conduction, in which a layer of gas without weight is included between two

plane solid sides having the constant temperatures 0 and *t*, we see that, if with decreasing density the mean length of path increases, the heat-flow must here change according to laws similar to those which govern the retarding force in friction. As in friction the velocities, so here the temperatures of a partition and the contiguous layer of gas differ by a finite value. A diminution, therefore, of the heat-flow must at first appear, as soon as this difference of temperature commences to exert a perceptible action. On further rarefaction a point is reached from which, onward, the idea of a coefficient of heat-conduction loses its meaning, and the flow of heat rapidly decreases as the density is diminished.

A comprehensive experimental testing of these consequences has hitherto not been possible with the apparatus we have employed. We have, however, succeeded here in doing approximately what we could not in the case of friction—namely, to produce a space which may be regarded as an actual vacuum in relation to heat-conduction. This made it possible, also, to separate the effects of conduction and radiation.

In order to measure the flow of heat through a gas, we observed (like Dulong and Petit) the cooling of thermometers of various forms in glass cases of different shapes at 0°. With higher pressures the action of pure heat-conduction is defaced by the action of the currents formed, in consequence of gravity, in the unequally heated gas. But when the pressure is lessened the velocity is increased with which, under given conditions, thermometric equilibrium is restored ; and therewith the influence of currents recedes. Thus, for a spherical thermometer, cooled in a spherical glass envelope, the time of cooling (from 60° to 20°) between 10 millims. and 1 millim. mercury-pressure is independent of the latter; on the contrary, with 150 millims. it was once and a half as great as with 750 millims. The values obtained between 10 millims. and 1 millim. mercury-pressure illustrate, on the one hand, the independence of the coefficient of heat-conduction of the pressure within those limits, and, on the other, can be utilized for calculating that coefficient. We find :—

Coefficient of heat-conduction.

	Observed.	Calculated by Maxwell.
Hydrogen . .	1	1
Air	0·137	0·141
Carbonic acid .	0·082	0·103

The coefficient of the conduction of heat of air in relation to that of hydrogen has been already found by Stefan in harmony

with Maxwell's theory; that of carbonic acid is, according to our experiments, sensibly less than the theoretic one.

The absolute conduction-coefficients of gases could only be approximately calculated, as we could procure only an approximate knowledge of the water-value of our thermometer. Using this approximate value, we find the conduction-coefficient of air equal to $\frac{10}{11}$ of that obtained by M. Stefan.

We will now describe how we succeeded in making the space of the cooling-apparatus employed approximately a vacuum with respect to the conduction of heat.

It appeared at first that the time of cooling of a thermometer, if a vacuum is produced in the well-dried cooling-space, diminishes with time when the apparatus is left to itself. For example, in one experiment, in twelve hours it fell from 351 seconds to 307 (in which times the thermometer sank from 60° to 20°). The traces of aqueous vapour which cause this may partly proceed from the fat of the cock; but they are partly discharged from the glass sides; for the discharge can be accelerated by heating parts of the latter. By such heating we were able to lessen the time of cooling in another exhausted apparatus from 282 to 264 seconds.

The quantities of aqueous vapour in question are extraordinarily small, and not to be indicated by the manometer. The velocity of cooling of a thermometer, which can be measured with great nicety and facility, consequently proves to be an extremely fine test for the quality of a vacuum; we doubt if a finer can be found.

We obtained the best vacua by drying the apparatus in the oil-bath at 200°, and shutting it off at that temperature. The following Table shows the gradual augmentation of the time of cooling.

Pressure. millim.	Time of cooling (60°–20°). seconds.
760	225
154	313
9·8	363
4	369
1·3	364
Vacuum I.	444
,, II.	555
,, III.	602
,, IV. (at 200°, dried, exhausted, and shut off)	712·5

By the last manipulation the time of cooling was increased in the ratio of 6 : 7.

In order now to prove whether a vacuum thus produced should

be regarded as an actual vacuum with respect to heat-conduction, we had a thermometer manufactured which, by means of slips of glass, could be put into two different envelopes. These were selected so that with medium pressures the times of cooling of the thermometer in them were in the ratio of 1 : 2. If now the best vacuum possible was produced in the envelopes, the result was that the times of cooling approached to within a few per cent. of equality.

Atmospheric Air.

$p.$ millim.	App. I. seconds.	App. II. seconds.
760 . . .	171	114
148 . . .	234	114
9·5 . . .	270	116
0·5 . . .	280	154
Best vacuum .	576	576

If the cooling-spaces were filled with hydrogen or carbonic acid and then exhausted as completely as possible, the results were :—

	App. I. seconds.	App. II. seconds.
Hydrogen vacuum . .	588	578
Carbonic-acid vacuum .	586	578

We thence concluded that, in these vacua, the quantities of heat abstracted from the thermometer proceeded almost exclusively from radiation. It is true it follows from the gas-theory that, from certain degrees of rarefaction onwards, the quantity of heat conducted is dependent only on the number of the molecules in the unit of space, and independent of the dimensions of the cooling-space; but with such degrees of rarefaction that quantity of heat is an extremely small fraction of that carried over with normal pressures.

We purpose to utilize such vacua for the determination of the absolute emissive power of black bodies and the dependence of its value on the temperature.

Using the water-value of our thermometer (to be sure only approximately ascertained), from our experiments up to this time a value of the emissive power of glass is obtained which nearly agrees with that ascertained by M. Lehnebach[*]; for many reasons, however, we lay no stress on this accordance.

On the dependence of emissive power on temperature we possess the law of Dulong and Petit, according to which its value is proportional to a^t, where a, referred to Centigrade, $=1\cdot0077$. But in the experiments on which Dulong and Petit founded that law the rarefied air of the cooling-space must,

* Pogg. *Ann.* vol. cli. p. 96.

according to our experience, have still possessed its full heat-conducting power. This being supposed, it follows from the dimensions of their apparatus, and from the emissive power of glass, that the ratio of the quantities of heat carried over by radiation and conduction in those experiments, for the temperature 100° of the thermometer, was in one series of experiments 6, and $2\frac{1}{2}$ in the other. Those experiments therefore cannot be looked upon as rigorously demonstrating the law founded upon them, the significance of which is moreover, according to the authors themselves, detracted from by the dependence of the specific heat of mercury on the temperature.

X. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xlix. p. 478.]

"ON the Refraction of Sound by the Atmosphere." By Professor Osborne Reynolds[*].

My object in this paper is to offer explanations of some of the more common phenomena of the transmission of sound, and to describe the results of experiments in support of these explanations. The first part of the paper is devoted to *the action of wind upon sound.* In this part of the subject I find that I have been preceded by Professor Stokes, who in 1857 gave precisely the same explanation as that which occurred to me. I have, however, succeeded in placing the truth of this explanation upon an experimental basis; and this, together with the fact that my work upon this part of the subject is the cause and foundation of what I have to say on the second part, must be my excuse for introducing it here. In the second part of the subject I have dealt with the effect of the atmosphere to refract sound upwards, an effect which is due to the variation of temperature, and which I believe has not hitherto been noticed. I have been able to show that this refraction explains the well-known difference which exists in the distinctness of sounds by day and by night, as well as other differences in the transmission of sound arising out of circumstances such as temperature; and I have applied it in particular to explain the very definite results obtained by Professor Tyndall in his experiments off the South Foreland.

The Effect of Wind upon Sound

is a matter of common observation. Cases have been known in which, against a high wind, guns could not be heard at a distance of 550 yards[†], although on a quiet day the same guns might be heard from ten to twenty miles. And it is not only with high winds that the effect upon sound is apparent; every sportsman knows how important it is to enter the field on the lee side even

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* Read April 23, 1874.   † Proc. Roy. Soc. 1874, p. 62.

when the wind is very light. In light winds, however, the effect is not so certain as in high winds; and (at any rate so far as our ears are concerned) sounds from a small distance seem at times to be rather intensified than diminished against very light winds. On all occasions the effect of wind seems to be rather against distance than against distinctness. Sounds heard to windward are for the most part heard with their full distinctness; and there is only a comparatively small margin between that point at which the sound is perceptibly diminished and that at which it ceases to be audible.

That sound should be blown back by a high wind does not at first sight appear to be unreasonable. Sound is known to travel forward through or on the air; and if the air is itself in motion, moving backwards, it will carry the sound with it, and so retard its forward motion—just as the current of a river retards the motion of ships moving up the stream. A little consideration, however, serves to show that the effect of wind on sound cannot be explained in this way. The velocity of sound (1100 feet per second) is so great compared with that of the highest wind (50 to 100 feet per second), that the mere retardation of the velocity, if that were all, would not be apparent. The sound would proceed against the wind with a slightly diminished velocity, at least 1000 feet per second, and with a but very slightly diminished intensity.

Neither can the effect of wind be solely due to its effect on our hearing. There can be no doubt that during a high wind our power of hearing is damaged; but this is the same from whatever direction the sound may come; and hence from this cause the wind would diminish the distance at which sounds could be heard, whether they moved with it or against it, whereas this is most distinctly not the case. Sounds at right angles to the wind are but little affected by it; and in moderate winds sounds can be heard further with the wind than when there is none.

The same may be said against theories which would explain the effect of wind as causing a heterogeneous nature in the air so that it might reflect the sound. All such effects must apply with equal force with and against the wind.

This question has baffled investigators for so long a time because they have looked for the cause in some direct effect of the motion of the air, whereas it seems to be but incidentally due to this. The effect appears, after all, not to be due simply to the wind, but to the difference in the velocity with which the air travels at the surface of the ground and at a height above it. That is to say, if we could have a perfectly smooth surface which would not retard the wind at all, then the wind would not obstruct sound in the way it does, for it would all be moving with an equal velocity; but, owing to the roughness of the surface and the obstructions upon it, there is a gradual diminution in the velocity of the wind as it approaches the surface. The rate of this diminution will depend on the nature of the surface; for instance, in a meadow the velocity at 1 foot above the surface is only half what it is at an elevation of

8 feet, and smaller still compared with what it is at greater heights.

To understand the way in which this variation in the velocity affects the sound, it is necessary to consider that the velocity of the waves of sound does depend on the velocity of the wind, although not in a great degree. To find the velocity of the sound with the wind we must add that of the wind to the normal velocity of sound, and against the wind we must subtract the velocity of the wind from the 1100 feet per second (or whatever may be the normal velocity of the sound) to find the actual velocity. Now, if the wind is moving at 10 feet per second at the surface of a meadow, and at 20 feet per second at a height of 8 feet, the velocity of the sound against the wind will be 1090 feet per second at the surface and 1080 feet per second at 8 feet above the surface; so that in a second the same wave of sound will have travelled 10 feet further at the surface than at a height of 8 feet. This difference of velocity would cause the wave to tip up and proceed in an upward direction instead of horizontally. For if we imagine the front of a wave of sound to be vertical to start with, it will, after proceeding for one second against the wind, be inclined at an angle of more than 45°, or half a right angle; and since sound-waves always move in a direction perpendicular to the direction of the front (that is to say, if the waves are vertical they will move horizontally and not otherwise), after one second the wave would be moving upwards at an angle of 45° or more. Of course, in reality, it would not have to proceed for one second before it began to move upwards: the least forward motion would be followed by an inclination of the front backwards, and by an upward motion of the wave. A similar effect would be produced in a direction opposite to that of the wind, only as the top of the wave would then be moving faster than the bottom, the waves would incline forwards and move downwards. In this way the effect of the wind is to lift the waves which proceed to windward, and to bring those down which move with it.

Thus the effect of wind is not to destroy the sound, but to raise the ends of the wave, which would otherwise move along the ground, to such a height that they pass over our heads.

When the ends of the waves are raised from the ground they will tend to diverge down to it, and throw off *secondary waves*, or, as I shall call them, *diverging waves*, so as to reconstitute the gap that is thus made. These secondary waves will be heard as a continuation of the sound, more or less faint, after the primary waves are altogether above our heads. [This phenomenon of divergence presents many difficulties, and has only as yet been dealt with for particular cases. It may, however, be assumed, from what is known respecting it, that in the case of sound being lifted up from the ground by refraction, or, what is nearly the same thing, passing directly over the crest of a hill so that the ground falls away from the rays of sound, diverging waves would be thrown off very rapidly at first and for a considerable distance, de-

pending on the wave-length of the sound; but as the sound proceeds further the diverging rays would gradually become fainter and more nearly parallel to the direct rays, until at a sufficient distance they would practically cease to exist, or, at any rate, be no greater than those which cause the diffraction-bands in a pencil of light*. The divergence would introduce bands of diffraction or interference within the direct or geometrical path of the sound, as in the case of light. These effects would also be complicated by the reflection of the diverging waves from the ground, which, crossing the others at a small angle, would also cause bands of interference. The results of all these causes would be very complicated, but their general effect would be to cause a rapid weakening of the sound at the ground from the point at which it was first lifted; and as the sound became weaker it would be crossed by bands of still fainter sound, after which the diverging rays, as well as the direct rays, would be lifted, and at the ground nothing would be heard.—*September* 1874.]

If we leave out of consideration the divergence, then we may form some idea as to the path which the bottom of the sound, or the rays of sound (considered as the rays of light), would follow. If the variation in the speed of the wind were uniform from the surface upwards, then the rays of sound would at first move upwards, very nearly in circles. The radii of these circles may be shown to be $1100 \times \dfrac{h}{v_1 - v_2}$, where v_1 and v_2 are the velocities of the wind in feet per second at elevations differing by h feet. In fact, however, the variation is greatest at the ground, and diminishes as we proceed upwards, so that the actual path would be more that of a parabola.

Also, owing to this unequal variation in the velocity, those parts of the waves immediately adjacent to the ground will rise more rapidly than the part immediately above them; hence there will be a crowding of the waves at a few feet from the ground, and this will lead to an intensifying of the sound at this point. Hence, notwithstanding the divergence, we might expect the waves to windward to preserve their full intensity so long as they were low enough to be heard. And this is in accordance with the fact, often observed, that sounds at short distances are not diminished but rather intensified when proceeding against the wind.

It will at once be perceived that by this action of the wind the distance to which sounds can be heard to windward must depend on the elevation of the observer and the sound-producing body. This does not appear to be a fact of general observation. It is difficult to conceive how it can have been overlooked, except that, in nine cases out of ten, sounds are not continuous, and thus do not afford an opportunity of comparing their distinctness at different places. It has often astonished me, however, when shooting, that a wind

* Taking sound of 1 foot wave-length, and comparing it with light whose wave-length is the 50,000th part of an inch, then the divergence of the sound at a mile from the point at which it left the ground would be comparatively the same as that of the light at $\frac{1}{10}$ of an inch from the aperture at which the pencil was formed.

which did not appear to me to make the least difference to the direction in which I could hear small sounds most distinctly, should yet be sufficient to cover one's approach to partridges, and more particularly to rabbits, even until one was within a few feet of them —a fact which shows how much more effectively the wind obstructs sound near the ground than even a few feet above it.

Elevation, however, clearly offered a crucial test whether such an action as that I have described was the cause of the effect of wind upon sound. Having once entertained the idea, it was clearly possible to put it to the test in this way. Also, if the principles hold in sound, something analogous must hold in the case of waves on the surface of a running stream of water—for instance, waves made near the bank of a river.

I had just reached the point of making such tests when I discovered that the same views had been propounded by Professor Stokes so long ago as 1857*. Of course, after such a discovery, it seemed almost unnecessary for me to pursue the matter further; but as there were one or two points about which I was not then quite certain, and as Prof. Stokes's paper does not appear to be so well known as it might be (I do not know of one writer on sound who has adopted this explanation), it still seemed that it might be well, if possible, to put the subject on an experimental basis. I therefore made the experiments I am about to describe; and I am glad that I did not rest content without them, for they led me to what I believe to be the discovery of refraction of sound by the atmosphere.

Fig. 1.
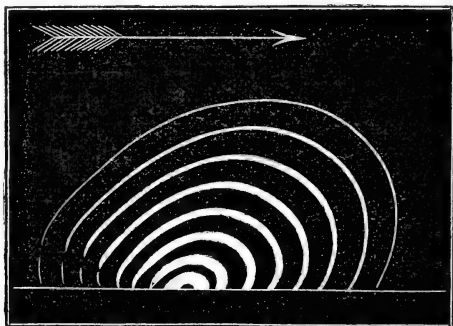
The results of my first observation are shown in fig. 1. This represents the shape of the waves as they proceeded outwards from a point near the bank of a stream about 12 feet wide. Had the water been at rest there would have been semicircular rings; as it was, the front of the waves up the stream made an obtuse angle with the wall, which they gradually left. The ends of the waves, it will be observed, gradually died out, showing the effect of divergence. The waves proceeding down the stream were, on the other hand, inclined to the wall, which they approached.

I was able to make a somewhat better observation in the Medlock, near the Oxford Road Bridge, Manchester. A pipe sent a succession of drops into the water at a few inches from the wall,

* Brit. Assoc. Report, 1857, Trans. of Sect. p. 22.

which, falling from a considerable height, made very definite waves.
Fig. 2 represents a sketch of these waves, made on the spot: the
diverging waves from the ends of the direct waves, and also the

Fig. 2.
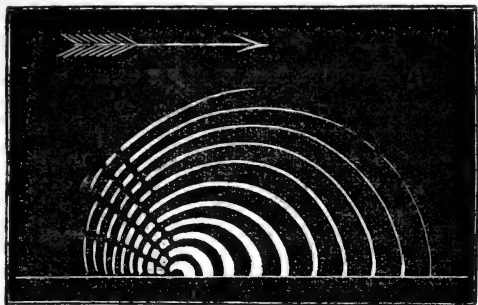
bands of interference, are very clearly seen. Both these figures agree
with what has been explained as the effect of wind on sound.

In the next place I endeavoured to ascertain the effect which
elevation has on the distance to which sound can be heard against
a wind. In making these experiments I discovered some facts
relating to the transmission of sound over a rough surface, which,
although somewhat obvious, appear hitherto to have escaped at-
tention.

My apparatus consisted of an electrical bell, mounted on a case
containing a battery. The bell was placed horizontally on the top
of the case, so that it could be heard equally well in all direc-
tions; and when standing on the ground the bell was 1 foot above
the surface. I also used an anemometer.

These experiments were made on four different days, the 6th,
9th, 10th, and 11th of March. On the first of these the wind was
very light, on the others it was moderately strong, strongest on
the second and fourth; on all four the direction was the same, viz.
north. On the two last days the ground was covered with snow,
which gave additional interest to the experiments, inasmuch as it
enabled me to compare the effect of different surfaces. On the
first two days I was alone, but on the last two I had the assist-
ance of Mr. J. B. Millar, of Owens College, whose ears were
rather better than mine, although I am not aware of any defi-
ciency in this respect. The experiments were all made in the
same place, a flat meadow of considerable extent.

The General Results of the Experiments.

De La Roche*, in his experiment, found that the wind produced
least effect on the sound at right angles to its direction, *i. e.* sounds
could be heard furthest in this direction. His method of experi-
menting, however, was not the same as mine. He compared the
sounds from two equal bells, and in all cases placed the bells at
such distances that the sounds were equally distinct. I, on the
other hand, measured the extreme distance at which the sounds
could be heard, the test being whether or not the observer noticed

* Annales de Chimie, vol. i. p. 177 (1816).

a break in the continuity of sound, a stoppage of the bell. The difference in our method of experimenting accounts for the difference in our results. I found in every case that the sound could be heard further with the wind than at right angles to its direction; and when the wind was at all strong, the range with the wind was more than double that at right angles. It does not follow, however, nor was the fact observed, that at comparatively short distances the sound with the wind was more intense than at right angles.

The explanation of this fact, which was fully borne out by all the experiments, is that the sound which comes in immediate contact with the ground is continually destroyed by the rough surface, and the sound from above is continually diverging down to replace that which has been destroyed. These diverging waves are in their turn destroyed; so that there is a gradual weakening of the intensity of the waves near the ground, and this weakening extends upwards as the waves proceed. Therefore, under ordinary circumstances, when there is no wind the distant sounds which pass above us are more intense than those which we hear. Of this fact I have abundant evidence. On the 6th, when the wind was light, at all distances greater than 20 yards from the bell the sound was much less at the ground than a few feet above it; and I was able to recover the sound after it had been lost in every direction by mounting on to a tree, and even more definitely by raising the bell on to a post 4 feet high, which had the effect of doubling the range of the sound in every direction except with the wind, although even in this the range was materially increased.

It is obvious that the rate at which the sound is destroyed by the ground will depend on the roughness of its surface. Over grass we might expect the sound at the ground to be annihilated, whereas over water it would hardly be affected. This was shown to be the case by the difference in the range at right angles to the wind over grass, and over the same ground when completely covered with snow. In the latter case I could hear the sound at 200 yards, whereas I could only hear it at 70 or 80 in the former.

Now, owing to the fact that the sound is greater over our heads than at the ground, any thing which slowly brings down the sound will increase the range. Hence, assuming that the action of the wind is to bring down the sound in the direction in which it is blowing, we see that it must increase its range in this direction. And it must also be seen that in this direction there will be less difference in the intensity of the sound from the ground upwards than in other directions. This was observed to be the case on all occasions. In the direction of the wind, when it was strong, the sound could be heard as well with the head on the ground as when raised, even when in a hollow with the bell hidden from view by the slope of the ground; and no advantage whatever was gained either by ascending to an elevation or raising the bell. Thus, with the wind over the grass the sound could be heard 140 yards, and over snow 360 yards, either with the head lifted or on the ground; whereas at right angles to the wind on all occasions the range was extended by raising either the observer or the bell.

It has been necessary to notice these points; for, as will be

seen, they bear directly on the question of the effect of elevation on the range of sound against the wind.

Elevation was found to affect the range of sound against the wind in a much more marked manner than at right angles.

Over the grass no sound could be heard with the head on the ground at 20 yards from the bell, and at 30 yards it was lost with the head 3 feet from the ground, and its full intensity was lost when standing erect at 30 yards. At 70 yards, when standing erect, the sound was lost at long intervals, and was only faintly heard even then ; but it became continuous again when the ear was raised 9 feet from the ground, and it reached its full intensity at an elevation of 12 feet.

Over the snow similar effects were observed at very nearly equal distances. There was this difference, however, the sound was not entirely lost when the head was lowered or even on the ground. Thus at 30 yards I could still hear a faint sound. Mr. Millar could hear this better than I could ; he, however, experienced the same increase on raising his head. At 90 yards I lost the sound entirely when standing on the ground, but recovered it again when the ear was 9 feet from the ground. Mr. Millar, however, could hear the sound very faintly, and at intervals, at 160 yards ; but not with his head on the ground. At this point I was utterly unable to hear it ; and even at an elevation of 25 feet I gave it up as hopeless. However, as Mr. Millar by mounting 10 feet higher seemed to hear it very much better, I again ascended ; and at an elevation of 33 feet from the ground I could hear it as distinctly as I had previously heard it when standing at 90 yards from the bell. I could not hear it 5 feet lower down ; so that it was the last 5 feet which had brought me into the foot of the wave. Mr. Millar experienced the same change in this 5 feet. As the sound could now be heard as strong as at a corresponding distance with the wind, we thought we had reached the full intensity of the waves. This, however, was not the case ; for the least raising of the bell was followed by a considerable intensifying of the sound ; and when it was raised 6 feet I could hear each blow of the hammer distinctly, although just at that time a brass band was playing in the distance. It seemed to me that I could hear it as distinctly as at 30 yards to leeward of the bell. All these results were repeated on both days with great uniformity.

When more than 30 yards to the windward of the bell, the raising of the bell was always accompanied by a marked intensifying of the sound, and particularly over the grass. I could only hear the bell at 70 yards when on the ground ; yet when set on a post 5 feet high I heard it 160 yards, or more than twice the distance. This is a proof of what I previously pointed out, that the waves rise faster at the ground than they do high up, and crowding together they intensify. In all cases there was an unmistakable greater distinctness of the sound from short distances of windward than to leeward or at right angles.

Except when the sound was heard with full force it was not uniform. The bell gave two sounds (the beats of the hammer and the ring) which could be easily distinguished ; and at times we

could hear only the ring, and at others the beats. The ring seemed to preserve itself the longest; whereas near the ground at short distances the ring was lost first. This is explained by the fact that the rate at which sound-waves diverge depends upon their note : the lower the note the more will they diverge. Thus the beats diverge more rapidly than the ring, and consequently die out sooner; whereas when the head is on the ground near the bell it is only the diverging waves that are heard, and here the beats have the best chance. The intensity of the sound invariably seemed to waver ; and as one approached the bell from the windward side, the sound did not intensify uniformly or gradually, but by fits or jerks ; this was the result of crossing the rays' interference, such as those shown in fig. 2.

During the observations the velocity of the wind was observed from time to time at points 1 foot and 8 feet above the surface.

On the 9th, that is over grass, it varied from 4 feet per second at 1 foot and 8 feet per second at 8 feet, to 10 feet at 1 foot and 20 feet at 8 feet, always having about twice the velocity at 8 feet that it had at 1 foot above the ground.

Over the snow there was not quite so much variation above and below. On the 10th the wind varied from 3 feet at 1 foot to 4 feet at 8 feet [*]. On the 11th the variation was from 12 at 1 foot and 19 at 8 feet to 6 at 1 foot and 10 at 8 feet. Thus over snow the variation in the velocity was only about one third instead of half.

Since the foregoing account was written, I have had an opportunity of experimenting on a strong west wind (on the 14th of March) ; and the results of these experiments are, if any thing, more definite than those of the previous ones. The wind on this occasion had a velocity of 37 feet per second at an elevation of 12 feet and of 33 at 8 feet and 17 at 1 foot. The experiments were made in the same meadows as before, the snow having melted, so that the grass was bare.

With the wind I could hear the bell at 120 yards, either with the bell on the ground or raised 4 feet above it. At right angles to the direction of the wind it ranged about 60 yards with the bell on the ground, and 80 yards when the bell was elevated.

To windward, with the bell standing on the ground (which, it must be remembered, means that the bell was actually 1 foot above the surface), the sound was heard as follows :—

	Full.	Lost.
With the head close to the ground	At 10 yards.	At 20 yards.
Standing	„ 30 „	„ 40 „
At an elevation of 25 feet	*Not heard* at 90 yards.	

With the bell at an elevation of 4 feet 6 inches :—

	Full.	Lost.
Head to the ground...........	At 18 yards.	At 30 yards.
Standing up	„ 40 „	„ 60 „
At an elevation of 12 feet	....	„ 90 „
At an elevation of 18 feet	„ 90 „	

These results entirely confirm those of the previous experiments;

[*] The wind fell rapidly towards the close of the observations on this day.

and the intensifying of the sounds to windward by the raising of the bell was even more marked than before; for at 90 yards to windward, with the bell raised, I could hear it *much* more distinctly than at a corresponding distance to leeward. This fact calls for a word of special explanation; it is clearly due to the fact that the variation in the velocity of the air is much greater near the ground than at a few feet above it. When the bell is on the ground all the sound must pass near the ground, and will all be turned up to a nearly equal extent; but when the bell is raised, the rays of sound which proceed horizontally will be much less bent or turned up than those which go down to the ground; and consequently, after proceeding some distance, these rays will meet or cross, and if the head be at this point they will both fall on the ear together, causing a sound of double intensity. It is this crossing of the rays also which for the most part causes the interference seen in fig. 2.

These experiments establish three things with regard to the transmission of sound :—

1. That when there is no wind, sound proceeding over a rough surface is more intense above than below.

2. That as long as the velocity of the wind is greater above than below, sound is lifted up to windward and is not destroyed.

3. That under the same circumstances it is brought down to leeward, and hence its range extended at the surface of the ground.

These experiments also show that there is less variation in the velocity of the wind over a smooth surface than over a rough one.

It seems to me that these facts fully confirm the hypotheses propounded by Prof. Stokes, that they place the action of wind beyond question, and that they afford explanations of many of the anomalous cases that have been observed ; for instance, that sounds can be heard much further over water than over land, and also that a light wind at sea does not appear to affect sound at all, the fact being that the smooth water does not destroy either the sound or the motion of the air in contact with it. When the wind and sea are rough the case is different.

The Effect of Variations of Temperature.

Having observed how the wind acts to lift the waves of sound by diminishing their velocity above compared with what is below, it was evident to me that any other atmospheric cause which would diminish the velocity above or increase that below would produce the same effect, viz. would cause the waves to rise.

Such a cause must at certain times exist in the variation in the condition of the air as we proceed upwards from the surface.

Although barometric pressure does not affect the velocity of sound, yet, as is well known, the velocity of sound depends on the temperature*, and every degree of temperature between 32° and 70° adds approximately 1 foot per second to the velocity of sound. This velocity also increases with the quantity of moisture in the air; but the quantity is at all times too small to produce an appreciable result. This vapour nevertheless plays an important part in

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* It varies as the square root of $\frac{\text{pressure}}{\text{density}}$, and consequently as the square root of the absolute temperature.

the phenomena under consideration; for it gives to the air a much greater power of radiating and absorbing heat, and thus renders it much more susceptible of changes in the action of the sun.

If, then, the air were all at the same temperature and equally saturated with moisture, the velocity of sound would be the same at all elevations; but if the temperature is greater, or if it contains more water below than above, then the wave of sound will proceed quicker below than above, and will be turned up in the same way as against a wind. This action of the atmosphere is, strictly speaking, analogous to the refraction of light. In light, however, it is density which retards motion; temperature and pressure have little or nothing to do with it; and since the density increases downwards, the rays of light move slower below than they do above, and are therefore bent downwards, and thus the distance at which we can see objects is increased. With sound, however, since it is temperature which affects the velocity, the reverse is the case; the rays are bent upwards, and the distance from which we can hear is reduced.

It is a well-known fact that the temperature of the air diminishes as we proceed upwards, and that it also contains less vapour. Hence it follows that, as a rule, the waves of sound must travel faster below than they do above, and thus be refracted or turned upward.

The variation of temperature is, however, by no means constant, and a little consideration serves to show that it will be greatest in a quiet atmosphere when the sun is shining. The sun's rays, acting most powerfully on that air which contains the most vapour, warms the lower strata more than those above them; and besides this, they warm the surface of the earth, and this warmth is taken up by the air in contact with it. It is not, however, only on such considerations as these that we are in a position to assert the law of variation of atmospheric temperature. Mr. Glaisher has furnished us with information on the subject which places it beyond the region of surmise.

I extract the following from his "Report on Eight Balloon Ascents in 1862" (Brit. Assoc. Rep. 1862, p. 462) :—

"From these results the decline of temperature when the sky was cloudy

For the first　300 feet was 0°·5 for every 100 feet.
From 300 to 3400　　,,　　0°·4　　,,　　,,
,, 3400 to 5000　　,,　　0°·3　　,,　　,,

"Therefore in cloudy states of the sky the temperature of the air decreased nearly uniformly with the height above the surface of the earth nearly up to the cloud.

"When the sky was partially cloudy the decline of temperature

In the first　　100 feet was 0°·9
　＊　　　　＊　　　　＊　　　　＊
From 2900 to 5000　　,,　　0°·3 for every 100 feet.

"The decline of temperature near the earth with a partially clear sky is nearly double that with a cloudy sky.

"In some cases, as on July 30th, the decline of temperature in the first 100 feet was as large as 1°·1."

We may say, therefore, that when the sky is clear the variation of temperature as we proceed upwards from 1 to 3000 feet will be more than double what it is when the sky is cloudy. And since for such small variations the variation in the velocity of sound, that is the refraction, is proportional to the temperature, this refraction will be twice as great with a clear sky as when the sky is cloudy.

This is the mean difference, and there are doubtless exceptional cases in which the variations are both greater and less than those given; during the night the variations are less than during the day, and again in winter than in summer.

This reasoning at once suggested an explanation of the well-known fact that sounds are less intense during the day than at night. This is a matter of common observation, and has been the subject of scientific inquiry. F. De La Roche discusses the subject, and exposes the fallacies of several theories advanced to account for it. Amongst others there are some remarks by Humboldt, in which he says that the difference is not due to the quietness of the night, for he had observed the same thing near the torrid zone, where the day seemed quieter than the night, which was rendered noisy with insects.

It is, however, by the experiments of Prof. Tyndall that this fact has been fully brought to light; and from their definite character they afford an opportunity of applying the explanation, and furnish a test of its soundness.

Neglecting the divergence of the bottom of the waves, a difference of 1 degree in the 100 feet would cause the rays of sound, otherwise horizontal, to move on a circle, the radius of which by the previous rule $= 1100 \cdot \frac{100}{1} = 110,000$ feet. A variation of one half this would cause them to move on a circle of 220,000 feet radius. From the radii of these circles we can calculate the range of the sound from different elevations.

With a clear sky, *i. e.* with a radius 110,000 feet from an elevation of 235 feet, the sound would be audible with full force to 1·36 mile; the direct sound would then be lifted above the surface, and only the diverging sound would be audible. From an elevation of 15 feet, however, the direct sound might be heard to a distance of ·36, or $\frac{1}{3}$ mile further, so that in all it could be heard 1·72 ($1\frac{3}{4}$) mile.

With a cloudy sky, *i. e.* with a radius 220,000 feet, the direct sound would be heard to 2·4 miles from an elevation of 15 feet, or 1·4 times what it is with the clear sky. These results have been obtained by taking the extreme variations of temperature at the surface of the earth. At certain times, however, in the evening, or when it was raining, the variation would be much less than this, in which case the direct sound would be heard to much greater distances.

[So far I have only spoken of the direct or geometrical rays of sound, that is, I have supposed the edge of the sound to be definite, and not fringed with diverging rays; but, as has been already explained, the sound would diverge downwards, and from this cause would be heard to a considerable distance beyond the point at which the direct rays first left the ground. From this point, however, the sound would become rapidly fainter until it was lost. The

extension which divergence would thus add to the range of the sound would obviously depend on the refraction—that is to say, when the direct rays were last refracted upwards, the extension of the range due to divergence would be greatest. It is difficult to say what the precise effect of this divergence would be ; but we may assume that it would be similar to that which was found in the case of wind, only the refraction being so much smaller the extension of the range by divergence would be greater. On the whole the results calculated from the data furnished by Mr. Glaisher agree in a remarkable manner with those observed ; for if we add $\frac{1}{4}$ mile for the extension of the range by divergence, the calculated distance with a clear sky would be two miles from a cliff 235 feet high.—*September* 1874.]

Now Prof. Tyndall found that from the cliffs at the South Foreland, 235 feet high, the minimum range of sound was a little more than 2 miles, and that this occurred on a quiet July day with hot sunshine. The ordinary range seemed to be from 3 to 5 miles when the weather was dull, although sometimes, particularly in the evening, the sounds were heard as far as 15 miles. This was, however, only under very exceptional circumstances. Prof. Tyndall also found that the interposition of a cloud was followed by an almost immediate extension of the range of the sound. I extract the following passages from Prof. Tyndall's Report :—

" On June 2 the maximum range, at first only 3 miles, afterwards ran up to about 6 miles.

" Optically, June 3 was not at all a promising day ; the clouds were dark and threatening, and the air filled with a faint haze ; nevertheless the horns were fairly audible at 9 miles. An exceedingly heavy rain-shower approached us at a galloping speed. The sound was not sensibly impaired during the continuance of the rain.

"July 3 was a lovely morning : the sky was of a stainless blue, the air calm, and the sea smooth. I thought we should be able to hear a long way off. We steamed beyond the pier end and listened. The steam clouds were there, showing the whistles to be active ; the smoke-puffs were there, attesting the activity of the guns. Nothing was heard. We went nearer ; but at two miles horns and whistles and guns were equally inaudible. This, however, being near the limit of the sound-shadow, I thought that might have something to do with the effect, so we steamed right in front of the station, and halted at $3\frac{3}{4}$ miles from it. Not a ripple nor a breath of air disturbed the stillness on board, but we heard nothing. There were the steam-puffs from the whistles, and we knew that between every two puffs the horn-sounds were embraced, but we heard nothing. We signalled for the guns ; there were the smoke-puffs apparently close at hand, but not the slightest sound. It was mere dumb-show on the Foreland. We steamed in to 3 miles, halted, and listened with all attention. Neither the horns nor the whistles sent us the slightest hint of a sound. The guns were again signalled for ; five of them were fired, some elevated, some fired point-blank at us. Not one of them was heard. We steamed in to two miles, and had the guns again fired : the

howitzer and mortar with 3-lb. charges yielded the faintest thud, and the 18-pounder was quite unheard.

" In the presence of these facts I stood amazed and confounded ; for it had been assumed and affirmed by distinguished men who had given special attention to this subject, that a clear, calm atmosphere was the best vehicle of sound : optical clearness and acoustic clearness were supposed to go hand in hand   *   *   *.

" As I stood upon the deck of the ' Irené' pondering this question, I became conscious of the exceeding power of the sun beating against my back and heating the objects near me.   Beams of equal power were falling on the sea, and must have produced copious evaporation.   That the vapour generated should so rise and mingle with the air as to form an absolutely homogeneous mixture I considered in the highest degree improbable.   It would be sure, I thought, to streak and mottle the atmosphere with spaces, in which the air would be in different degrees saturated, or it might be displaced by the vapour.   At the limiting surfaces of these spaces, though invisible, we should have the conditions necessary to the production of partial echoes, and the consequent waste of sound.

" Curiously enough, the conditions necessary for the testing of this explanation immediately set in.   At 3.15 P.M. a cloud threw itself athwart the sun, and shaded the entire space between us and the South Foreland.   The production of vapour was checked by the interposition of this screen, that already in the air being at the same time allowed to mix with it more perfectly ; hence the probability of improved transmission.   To test this inference the steamer was turned and urged back to our last position of inaudibility.   The sounds, as I expected, were distinctly though faintly heard.   This was at 3 miles distance.   At $3\frac{3}{4}$ miles we had the guns fired, both point-blank and elevated.   The faintest thud was all that we heard ; but we did hear a thud, whereas we had previously heard nothing, either here or three quarters of a mile nearer.   We steamed out to $4\frac{1}{4}$ miles, when the sounds were for a moment faintly heard, but they fell away as we waited ; and though the greatest quietness reigned on board, and though the sea was without a ripple, we could hear nothing.   We could plainly see the steam-puffs which announced the beginning and the end of a series of trumpet-blasts, but the blasts themselves were quite inaudible.

" It was now 4 P.M., and my intention at first was to halt at this distance, which was beyond the sound-range, but not far beyond it, and see whether the lowering of the sun would not restore the power of the atmosphere to transmit the sound.   But after waiting a little, the anchoring of a boat was suggested ; and though loth to lose the anticipated revival of the sounds myself, I agreed to this arrangement.   Two men were placed in the boat, and requested to give all attention, so as to hear the sound if possible.   With perfect stillness around them, they heard nothing.   They were then instructed to hoist a signal if they should hear the sounds, and to keep it hoisted as long as the sounds continued.

" At 4.45 we quitted them and steamed towards the South Sand

Head light-ship. Precisely fifteen minutes after we had separated from them the flag was hoisted. The sound, as anticipated, had at length succeeded in piercing the body of air between the boat and the shore.

"On returning to our anchored boat, we learned that when the flag was hoisted the horn-sounds were heard, that they were succeeded after a little time by the whistle-sounds, and that both increased in intensity as the evening advanced. On our arrival of course we heard the sounds ourselves.

"The conjectured explanation of the stoppage of the sounds appeared to be thus reduced to demonstration; but we pushed the proof still further by steaming further out. At $5\frac{3}{4}$ miles we halted and heard the sounds. At 6 miles we heard them distinctly, but so feebly that we thought we had reached the limit of the sound-range; but while we waited the sound rose in power. We steamed to the Varne buoy, which is $7\frac{3}{4}$ miles from the signal-station, and heard the sounds there better than at 6 miles distance.

"Steaming on to the Varne light-ship, which is situated at the other end of the Varne shoal, we hailed the master, and were informed by him that up to 5 P.M. nothing had been heard. At that hour the sounds began to be audible. He described one of them as 'very gross, resembling the bellowing of a bull,' which very accurately characterizes the sound of the large American steam-whistle. At the Varne light-ship, therefore, the sounds had been heard towards the close of the day, though it is $12\frac{3}{4}$ miles from the signal-station."

Here we see that the very conditions which actually diminished the range of the sound were precisely those which would cause the greatest lifting of the waves. And it may be noticed that these facts were observed and recorded by Prof. Tyndall with his mind altogether unbiased with any thought of establishing this hypothesis. He was looking for an explanation in quite another direction. Had it not been so he would probably have ascended the mast, and thus found whether or not the sound was all the time passing over his head. On the worst day an ascent of 30 feet should have extended the range nearly $\frac{1}{4}$ mile.

The height of the sound-producing instruments is apparently treated as a subordinate question by Prof. Tyndall. At the commencement of his lecture, he stated that the instruments were mounted on the top and at the bottom of the cliff; and he subsequently speaks of their being 235 feet above him. He does not, however, take any notice of the comparative range of those on the top and those at the bottom of the cliff; but wherever he mentions them he speaks of them as on the cliff, leading me to suppose that for some reason those at the bottom of the cliff had been abandoned, or that they were less efficient than those above. If I am right in this surmise, if the sounds from below did not range so far as those from above, it is a fact in accordance with refraction, but of which, I think, Prof. Tyndall has offered no explanation.

[Besides the results of Prof. Tyndall's experiments there are many other phenomena which are explained by this refraction. Humboldt could hear the falls of Orinoco three times as loud by

night as by day at a distance of one league; and he states that the same phenomenon has been observed near every waterfall in Europe. And although Humboldt gave another explanation*, which was very reasonable when applied to the particular case at Orinoco†, yet it must be admitted that the circumstances were such as would cause great upward refraction; and hence there can be but little doubt that refraction had a good deal to do with the diminution of the sound by day.

In fact if this refraction of sound exists, then, according to Mr. Glaisher's observations, it must be seldom that we can hear distant sounds with any thing like their full distinctness, particularly by day; and any elevation in the observer or the source of the sound above the intervening ground will increase this range and distinctness, as will also a gentle wind, which brings the sound down and so counteracts the effect of refraction. And hence we have an explanation of the surprising distances to which sounds can sometimes be heard, particularly the explosion of meteors, as well as a reason for the custom of elevating church-bells and sounds to be heard at great distances.—*September* 1874.]

XI. *Intelligence and Miscellaneous Articles.*

ON THE ACTION OF MAGNETS ON RAREFIED GASES IN CAPILLARY TUBES RENDERED LUMINOUS BY AN INDUCED CURRENT. BY J. CHAUTARD.

THE spectral modifications produced by the action of magnets on the light of an induced current traversing rarefied gases are subject to very complex laws; and it is only possible to formulate them after varied and long-continued experiments. M. Trève, in a Note published in the *Comptes Rendus* (Jan. 3, 1870), after indicating some facts bearing on this class of phenomena, concluded in these terms—" coloration and decoloration of the gases under the action of magnetism, in the capillary parts of the tubes containing them;" but the experiments of the accomplished officer were not very numerous, they were made on only a few gases, and appeared to be only indirectly connected with the researches he had undertaken. The subject seemed to me sufficiently interesting to

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* " That the sun acts upon the propagation and intensity of sound by the obstacles met in currents of air of different density, and by the partial undulations of the atmosphere arising from unequal heating of different parts of the soil. . . . . During the day there is a sudden interruption of density wherever small streamlets of air of a high temperature rise over parts of the soil unequally heated. The sonorous undulations are divided, as the rays of light are refracted wherever strata of air of unequal density are contiguous. The propagation of sound is altered when a stratum of hydrogen gas is made to rise over a stratum of atmospheric air in a tube closed at one end; and M. Biot has well explained, by the interposition of bubbles of carbonic acid gas, why a glass filled with champagne is not sonorous so long as that gas is evolved and passing through the strata of the liquid."—*Humboldt's Travels*, Bohn's Series, vol. ii. p. 264.

† The sounds proceeded over a plane covered with rank vegetation interspersed with black rocks. These latter attained a considerable elevation of temperature under the effects of the tropical sun, as much as 48° C., while the air was only 28°; and hence over each rock there would be a column of hot air ascending.

be the object of a fresh study, of which I have today the honour to present to the Academy a rapid summary.

Conditions of the Experiments.—Without returning to the experimental arrangements indicated in my first Note*, I shall briefly analyze those which have permitted me to extend, and at the same time give a precise account of, my fresh experiments. These are:—the nature, temperature, and pressure of the gas; the intensity, direction, and source of the induced current; the action of the magnet through the form of its poles, the energy and direction of the magnetization, the distance of the armatures, and the axial or equatorial position of the tube containing the gas†.

(1) The rarefied gases or substances on which my experiments have been made are hydrogen, nitrogen, oxygen, carbonic acid, carbonic oxide, bicarburetted hydrogen, sulphur, selenium, iodine, bromine, chlorine, sulphurous acid, fluoride of silicium, bichloride of tin. All of them are far from presenting very pronounced modifications, as I shall presently show; the substances of the chlorine group are those most sure to succeed, and produce the most brilliant results.

(2) Elevation of temperature lessens the effect produced by the magnet. This can be ascertained by causing the induced current to pass for some time within the tube: the heat resulting soon weakens and sometimes renders inactive the magnetic influence.

(3) The pressure of the gas interferes with the action of the magnet to such a degree that it is possible with the same substance to obtain, according to the conditions, either the sudden cessation of the induced current, or a notable modification in the luminous appearance, or the permanence of the initial tint.

(4) By varying the intensity of the induced current, effects can be obtained similar to those which result from varying the pressure of the gas: in general, the more feeble the initial intensity, the more decided are the magnetic luminous modifications.

(5) The phenomena are the same when the induced current is derived from a Holtz machine or a Ruhmkorff induction-coil.

(6) Both directions of the induced current, as also of the magnetization, give pretty nearly identical effects; certain substances, however, seem to undergo a more energetic influence at the moment of the reversal of the current.

(7) In the form of the armatures, the surface ought chiefly to be considered; this should be plane, and such that the capillary tube will be embraced over the greater part of its length.

(8) It is evident that the more energetic the magnetization, the more pronounced will the phenomena be; it is usually determined with the aid of a pile of 12–15 Bunsen elements (large pattern).

(9) Lastly, the action diminishes rapidly with distance; this is ascertained by gradually removing the tube to about $\frac{1}{2}$ centim. from the poles; beyond that limit the influence of the magnet ceases to be manifest.

Conclusions.—(1) The first result to be noted is an increase of

* *Comptes Rendus,* Nov. 16, 1874, p. 1123.

† The form of my apparatus has not, up to the present time, enabled me to compare the effects resulting from these two positions.

resistance of the part of the induced current under the influence of the magnet. This resistance is sometimes such that the current may be suddenly interrupted at the instant when the magnet begins to act. This is made evident in the following manner. A tube is taken formed of two parts in communication, one of them presenting a constriction, the other a different length and diameter. The capillary part is placed in the pole of the electromagnet, after which the current of the coil is started. As long as the magnet is inactive, the light circulates uniformly in the two tubes; it is suddenly arrested in the shortest and narrowest at the instant when this is submitted to the action of the magnet. The effect can be produced with chlorine, iodine, sulphur, selenium.

(2) This cessation of the induced light caused by the magnet can be determined, with the same gas, in two quite distinct cases—either when exhaustion has been carried so far that the induction-current is near the limit which no longer permits it to pass, or, on the contrary, when the tension of the gas is sufficient for the spark to be near the same limit.

(3) Under the magnetic influence the luminous thread, when it persists, undergoes in capillary tubes a narrowing which can sometimes be perceived by simple inspection. This narrowing is produced by an augmentation of resistance, sufficiently energetic at times to be accompanied by a change of tint in the tube, or even by a modification of the spectrum. In certain gases, such as hydrogen, nitrogen, carbonic acid, the influence of the magnet is hardly perceptible, and the modifications observed enter into the system of the primitive lines.

(4) This narrowing, or the change of tint of the luminous thread, does not extend to more than half a centimètre from the poles: thus on taking a tube of sufficient length, by changing the height of the spectroscope while the magnetization is going on, the normal spectrum (that produced by the light outside of the magnetic field) and the spectrum modified by the vicinity of the magnet can be successively seen.

(5) In order to form a good judgment of the action of the magnet, it is necessary to manage so that the spectrum is not very bright at starting. As soon as the current passes in the electromagnet, the lines appear in all their splendour. The phenomenon is particularly successful and gives the most perspicuous results with chlorine, bromine, chloride of tin, fluoride of silicium, and sulphuric acid.

(6) Direct measurements have proved that, for these last substances, the new lines developed under these circumstances are distinct from those which characterize the normal spectrum of the same gas traversed by a sufficiently energetic induced current outside the range of a magnet.—*Comptes Rendus de l'Acad. des Sciences,* vol. lxxx. pp. 1161–1164.

ON THE VELOCITY OF MAGNETIZATION AND DEMAGNETIZATION OF IRON, CAST IRON, AND STEEL. BY M. DEPREZ.

In pursuing my researches on electromagnets and their application to the registration of very rapid phenomena, the first results of which have been already communicated to the Academy, I have

been led to investigate what was the effect of the nature of the iron of the electromagnet upon the duration of the phases of magnetization and demagnetization. For this purpose I have employed a register in which the pieces of iron constituting the electromagnet are removable, the bobbins, armature, style, &c. remaining the same, so as to make evident the influence of the metal of the electromagnet. To measure the duration of the phases, I have made use of the method indicated in my first communciation on electric chronographs.

The metallic portion of the electromagnets, which I placed successively in the magnetizing helices, consisted of two cores of 2 millims. diameter and 13 millims. length. The coils into which the current passed contained 14 mètres of wire $\frac{1}{5}$ millim. in diameter. The pile consisted of one Bunsen's element modified by M. Dulaurier. The varieties of iron tried were the ordinary iron of commerce, the soft iron used specially for telegraphs, malleable cast iron, grey cast iron, and cast steel stretched and chilled.

The results obtained were quite unexpected ; for the soft iron, ordinary iron, malleable cast iron, and *even the chilled steel* gave very nearly the same results for the duration of the phases of magnetization and demagnetization, viz.

<div style="text-align:right">second.</div>

Duration of demagnetization . . . . 0·00025

,,   magnetization (approximately) 0·00150

The grey cast iron gave still better results ; for the duration of the magnetization was reduced to about $\frac{1}{1000}$ of a second. It would therefore be this last metal that would permit the greatest possible rapidity to be obtained in the transmission of signals.

In brief, with my present registers (such as will be described soon) signals can be obtained perfectly distinct, succeeding each other at intervals of $\frac{1}{350}$ of a second, by employing no matter what sort of iron for the electromagnets, and of $\frac{1}{500}$ of a second when the latter are of grey cast iron. It is essential to remark that I am not speaking here of signals *succeeding each other regularly* at intervals of $\frac{1}{350}$ or $\frac{1}{500}$ of a second, which would constitute a system. In this latter case, indeed, the number of signals which can be transmitted exceeds by far 350 or 500 per second.

I am disposed to believe that the superiority of cast iron depends on its molecular texture, and not on the quantity of carbon which it contains. I intend also to try *soft iron cast and not forged,* which, I think, will exceed in rapidity all that I have hitherto obtained. I purpose, moreover, in another communication, shortly to revert to the details of my experiments, and to the application of my registers to electric chronographs intended for artillery.

The durations above indicated, it must be observed, do not include the time employed by the style in traversing its trajectory ; it is by adding this to the durations of magnetization and demagnetization that $\frac{1}{350}$ or $\frac{1}{500}$ of a second, according to the case, is found for the *total* duration of a signal, comprising the demagnetization, the time of fall of the style, the magnetization, and the return of the style to its initial position. Besides, these are the numbers when only one pile-element is employed ; the number of signals transmitted per second increases with the intensity of the current.—*Comptes Rendus de l'Acad. des Sciences,* vol. lxxx. p. 1353

XII. *On a New Form of Micrometer for use in Spectroscopic Analysis. By* W. M. WATTS, *D.Sc., Physical-Science Master in the Giggleswick Grammar School*.*

IN determining the position of lines in a spectrum by the use of a micrometer-eyepiece or divided arc, it is often difficult to see the cross-wires distinctly without admitting extraneous light, which with faint spectra frequently cannot be done. I have sought to overcome this difficulty by substituting some one known line of the spectrum itself for the cross-wires, and thus to measure the position of unknown lines, by bringing this index-line successively into coincidence with them. Thus, for example, the sodium-line (which is present in nearly every spectrum whether it is wanted or not) may be made to move along under the spectrum, and the displacement necessary to make it coincide with the lines to be measured may be determined by the readings of a micrometer-screw. To accomplish this a convex lens of about 2 feet focus is placed in front of the prism of the spectroscope, between the prism and the observing-telescope, and is divided along a line at right angles to the refracting edge of the prism. One half of the lens is fixed; the other half is made to slide over it by means of a micrometer-screw. When the movable half of the lens is in its normal position, the only effect is to alter the focus of the telescope slightly; but when it is made to slide over the fixed half, the refraction of the prism is increased or diminished, half of the spectrum appears to move over the other half, and the sodium-line, or any other con-

venient line of reference, can be brought into coincidence with the lines to be measured.

The figure represents the micrometer as applied to a direct-vision spectroscope of   Browning.   *a* and *b* are the two strips
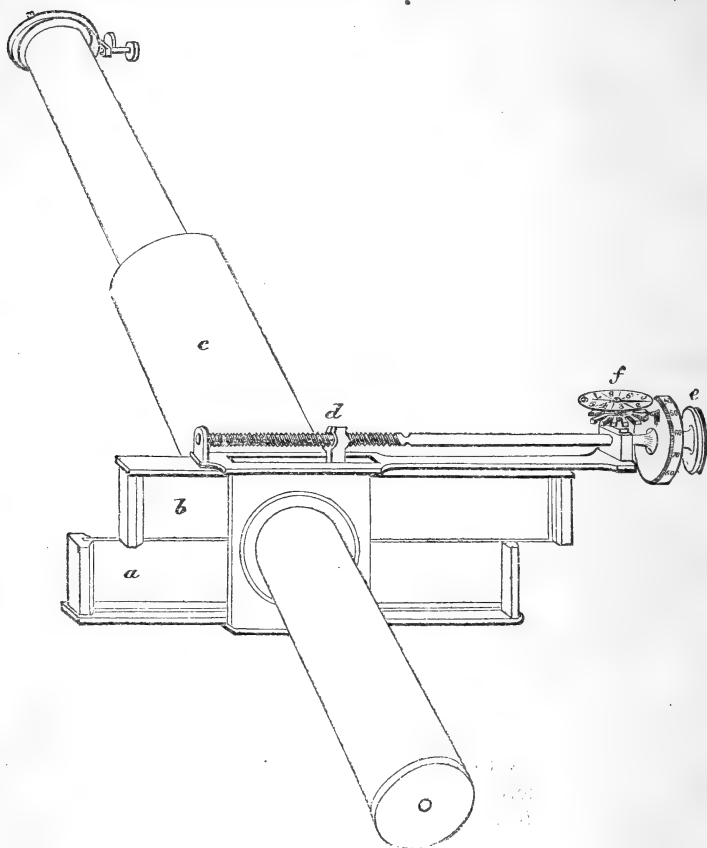
cut from a lens of about 2 feet focus, placed in front of the compound prism contained in the body *c*.   Of these *a* is fixed; but *b* can be made to slide over *a* by means of a screw *d*; and the displacement is measured by means of the graduated head *e* and the counter *f*.

In order to reduce the indications of this instrument to wavelengths, I have constructed a series of interpolation-curves from the data obtained by careful observation of the solar spectrum. One of these curves is for use when the sodium-line ($D_1 = 5889$) is employed as the reference-line, another when the Fraunhofer

line b ($b_1 = 5183$) is used, and so on. For use with terrestrial spectra of bright lines, the lithium-, sodium-, and thallium-lines, the hydrogen-lines obtained from a vacuum-tube, or for faint spectra the carbon-lines obtained from a Bunsen burner, furnish convenient reference-lines in·sufficient number. The curves are drawn on such a scale that a difference in wave-length of 100 tenth-metres* is represented by four inches, and one turn of the micrometer-screw is represented by one inch.

It requires 21·91 turns of the screw to bring the sodium-line (5892) into coincidence with the thallium-line (5349). The interpolation-curves are very regular and of slight curvature. In fact the readings of this micrometer may be treated in exactly the same way as the readings obtained by means of a graduated arc or bifilar eyepiece, or any of the other forms of measuring-apparatus.

Within the limits allowed by the construction of the instrument, the displacements measured by the micrometer-screw necessary to bring a given reference-line into coincidence with other lines to be measured are proportional to the angles between these lines and the reference-line as they would be obtained by a telescope moving over a graduated arc. The extreme displacement of which the upper half of the lens is capable is 0·69 of an inch; and assuming a refractive index of 1·5 for the glass of which the lens is made, the radius of its surface is 23 inches. The deviation of a ray of light passing through the lens in its extreme position is thus that due to a prism of refracting angle $2 \sin^{-1} \dfrac{0·69}{23·00}$; that is, about twice the angle whose sine is 0·03. If, now, we calculate the minimum deviation which a ray of light suffers in passing through the lens at points whose distances from the axis of the lens are represented by 0·01, 0·02, and 0·03 respectively, we obtain angles very nearly in the ratio of these numbers.

The following numbers show the deviation suffered by a ray of light in passing through a glass prism ($\mu = 1·5$) of varying angle:—

Angle of prism.	Deviation.
$2 \sin^{-1} 0·01$	0 34 22
$2 \sin^{-1} 0·02$	1 8 46
$2 \sin^{-1} 0·03$	1 43 12

The angles 0° 34′ 23″, 1° 8′ 46″, 1° 43′ 9″ would be as the numbers 1, 2, 3.

* A tenth-metre is $\left(\dfrac{1}{10}\right)^{10}$ of a metre, or the ten-millionth of a milli-metre.

I add the results of a few measurements made with the new micrometer, designed to test the accuracy of which it is capable. Twenty different readings of the point at which there is coincidence of the lenses gave the following results :—

8·36 8·29 8·25 8·32 8·36 8·34 8·38 8·37 8·34 8·37
Mean 8·35

8·37 8·41 8·26 8·28 8·31 8·35 8·21 8·35 8·38 8·35
Mean 8·33

To bring the sodium-line into coincidence with the thallium-line the following displacements were necessary :—

21·90   21·90   21·89   21·90   Mean 21·90
21·92   21·93   21·94   21·88   Mean 21·92

I have generally taken the mean of four readings as giving the true position of a line.

The light emitted by burning magnesium examined with the spectroscope shows, besides the bright lines corresponding to *b*, a series of bands nearer to F.   I had measured the wave-lengths of these bands with the greatest accuracy I could attain by the use of Browning's 6-prism automatic spectroscope in conjunction with a bifilar micrometer-eyepiece.   The reference-lines employed were the bright lines of the spark due to zinc, cadmium, iron, and air, and certain Fraunhofer lines.   The following results were obtained for the *first* and *second* bands :—

First band.   5006·5 from Fraunhofer lines.
5009·0 from zinc-line   4924
5008·0 from air-lines $\begin{cases} 5002 \\ 5005 \end{cases}$
5006·5 from Fraunhofer lines.
5007·7 from air-lines $\begin{cases} 5002 \\ 5005 \end{cases}$

Second band.   4995·0 from Fraunhofer lines.
4997·0 from air-lines $\begin{cases} 5002 \\ 5005 \end{cases}$
4997·4 from air-lines $\begin{cases} 5002 \\ 5005 \end{cases}$

The numbers finally adopted are given below for the whole nine bands ; and with them are compared the results recently obtained with the new micrometer.   The reference-line employed for these last measurements was the bright line corresponding to *b* present in the same spectrum.

Wave-lengths of Lines of Magnesium Oxide.

Automatic spectroscope with micrometer-eyepiece.	Direct-vision spectroscope with new micrometer.
5007	5006·0
4997	4996·0
4986	4985·5
4975	4974·5
4963	4963·5
4948	4948·5
4934	4934
*4918?	4924
	4914

Each of these results is the mean of five readings. A re-measurement of the first two bands by displacement of the lines in the opposite direction gave 5006·2 and 4996·1.

To test the capabilities of the new micrometer on faint spectra, two determinations were made of the wave-length of a line in the oxide-of-carbon spectrum given by a vacuum-tube enclosing coal-gas. The wave-length of this line I found before by measurement with a three-prism instrument to be 5195. It was obtained very faintly by a feeble discharge through the tube, and was measured by using as reference-line the lines given by a Bunsen flame.

Two successive determinations (each the mean of four readings) gave 5196 and 5196.

The advantages of the new form of micrometer seem to be (1) great precision in results, (2) convenience in use. My thanks are due to Mr. Browning for the skill with which he carried out my wishes in the construction of the instrument.

XIII. *On some Phenomena connected with the Boiling of Liquids.* By CHARLES TOMLINSON, *F.R.S.*

THE boiling-point of a liquid in an open vessel may be defined as the lowest temperature at which its vapour can have the elasticity of common air. But the temperature of the liquid itself and that of its vapour under the terms of the definition are not necessarily identical. Many circumstances, irrespective of pressure, tend to raise or lower the temperature of the liquid, such as (1) the nature of the vessel, since the liquid may wet some surfaces more or less completely, and others not at all ; (2) the state of such surfaces with regard to chemical

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* Too faint to be measured exactly.

cleanliness ; (3) the presence or absence of nuclear points in such surfaces; (4) the presence of nuclei purposely introduced into the liquid; (5) the presence of air in the liquid as diminishing cohesion ; (6) the purity or impurity of the liquid as influencing cohesion ; (7) the mode of applying heat to the vessel containing the liquid.

I take, for example, a specimen of wood-spirit of which the boiling-point is 148° F.*   In a clean test-tube over the flame of a spirit-lamp, with a clean thermometer held in the axis of the tube, the boiling agrees exactly with this temperature; but in a clean glass vessel plunged into a water-bath at 208°, the temperature of the liquid may rise to 169° or 21° above its boiling-point without any appearance of boiling.   Distillation, however, is going on from the surface, as may be shown by conducting the experiment in a thin glass retort connected with a glass condensing receiver. If the thermometer be raised out of the liquid into the vapour immediately above its surface the temperature falls rapidly to 148°.   Or if a bit of cocoanut-shell charcoal weighing only 3 or 4 grains be put into the retort, the liquid boils briskly at 148°, and the temperature does not rise, even though the belly of the retort be plunged into water at 210° F.   If, however, the operation be long continued, both the liquid and the vapour of low-boiling fluids may become slightly superheated even with a charcoal nucleus.   In the absence of such nucleus, the superheating both of liquid and vapour is considerable, as will be seen further on.

The amount of vapour given off under the two last-named conditions, namely the presence or the absence of a nucleus, varies greatly.   An ounce and a half of the same wood-spirit containing a chip of cocoanut-shell charcoal weighing 4 grains, was distilled during six minutes in a water-bath at 198° F. ; and the weight of the distillate was 273 grains.   In this case the temperature of the liquid scarcely rose above 148° during the operation ; but in a second operation under the same conditions, except that the charcoal was omitted, the wood-spirit quickly rose to 162° or 163°, and the weight of the distillate was only 150 grains, or 123 grains less than in the former case, in which the charcoal nucleus was present.

Seeing, then, how powerful a nucleus cocoanut-shell charcoal is, and how usefully it may be employed in facilitating the operations of boiling and distilling, it is a matter as well of technological as of scientific interest to determine, if possible, its function, and to show the part that it really performs in facilitating so greatly

* In Watts's Dictionary, under *Methyl, Hydrate of* (iii. 988), it is stated that wood-spirit boils at from 60° to 66°·5 C. (140° to 151°·7 F.), according to the nature of the vessel, and bumps strongly during boiling.

the separation of vapour from boiling liquids, producing regular ebullition, and preventing *soubresauts*, which endanger the safety of the vessel if not of the operator.

If, as M. Gernez asserts, the function of nuclei be simply that of carrying down air into the liquid, that function must soon become exhausted. Indeed M. Gernez attempts to show that any nucleus can be exhausted, not readily it is true, but under the influence of repeated boiling and reboilings separated by short intervals of time. I showed in my last paper that cocoa-nut-shell charcoal and some other nuclei cannot be thus ex-hausted when the mode of applying heat is by means of the flame of a spirit-lamp. But, it may be asked, suppose the mode of heating be by means of a hot bath, what then is the effect of repeated boiling? The results of experiments on this point seem to me to be as remarkable as they are instructive.

If, as M. Gernez affirms, a nucleus, such as charcoal, becomes inactive after boiling five or from that to ten times in a liquid at intervals of five minutes or so, the charcoal ought to show a gradual decline in power, from its highest state of efficiency, through advancing degrees of feebleness, to inactivity. In order to test this point, wood-spirit was distilled a number of times in the presence of a bit of cocoanut-shell charcoal weighing 5 grains; and the distillate was weighed each time after the opera-tion had been conducted during five minutes. The source of heat was a beaker containing $10\frac{1}{2}$ ounces of water, which was raised to boiling by the flame of a large spirit-lamp, after which a smaller flame was kept under the beaker to prevent the tem-perature from falling too much in consequence of the introduc-tion of the cold vessel containing the wood-spirit, $1\frac{1}{2}$ ounce of which was put into a small deep-bellied retort, the beak of which passed into a globular glass receiver; and this was kept wet with cold water. A thermometer passing through a cork closed the tubulure of the retort, while another thermometer was in the water-bath.

> Water-bath containing $10\frac{1}{2}$ oz. of water.
> Condenser   ,,   8   ,,
> Wood-spirit boiling at 148° F. $1\frac{1}{2}$ oz.
> Cocoanut-shell charcoal 5 grains.

First operation.—Bath 208°. The charcoal became active when the spirit was at 90° or 100°; and it was soon dragged upwards by the multitude of bubbles that issued from it. The temperature of the spirit quickly attained 148°, at which it re-mained until near the close of the operation. The introduc-tion of the retort lowered the temperature of the bath.

Bath 195° F.   Spirit 148° F.
          192   148
          188   148
          186   150

Weight of distillate in five minutes 194 grains.

Second operation.—After an interval of five minutes a fresh charge of wood-spirit was introduced into the retort. The charcoal was not fully active until the spirit was near its boiling-point; it then became very active.

Bath 195°   Spirit 148½°
          192   150
          190   151

Weight of distillate in five minutes 192 grains.

Third operation.—After five minutes with a fresh charge. The charcoal became active at 140°.

Bath 196°   Spirit 148°
          194   148
          192   148

Weight of distillate in five minutes 212 grains.

Fourth operation.—After five minutes with a fresh charge. The charcoal became active at 148°.

Bath 198°   Spirit 148°
          194   150
          192   150
          190   149½

Weight of distillate in five minutes 208 grains.

Fifth operation.—After an hour with a fresh charge. The charcoal which had remained in the spirit during the hour's interval became active at 140°.

Bath 202°   Spirit 148°
          196   150
          194   151

Weight of distillate in five minutes 182 grains.

Sixth operation.—After twenty-three hours, during which the charcoal was left in the spirit in the retort. It became active at 148°.

Bath 196°   Spirit 151°
          193   152
          190   153

Weight of distillate in five minutes 216 grains.

Seventh operation.—After an interval of twenty-four hours, during which the charcoal had been left in the spirit. In this operation the charcoal did not become active until the spirit had

acquired a temperature of 160°, or 12° above its boiling-point; it then suddenly discharged so much vapour as to turn about half the contents of the retort into the receiver.

Eighth operation.—Similar to the seventh. The spirit rose to 161°, when the charcoal suddenly became active, and the spirit sank to 150°.

Ninth operation.—After five minutes with a fresh charge. The charcoal burst into activity at 148°, and the distillation proceeded regularly.

<div align="center">

Bath 196°   Spirit 152°
193   153

</div>

Weight of distillate in five minutes 190 grains.

The above details seem to me to throw considerable light on the question at issue, namely, what is the function of such a nucleus as cocoanut-shell charcoal? We have seen that in the first operation a fresh piece of charcoal becomes active at 90° or 100° when the air contained within its pores becomes displaced by the heat and by the vapour; and it soon displays great activity in liberating vapour, and would continue to be active during many hours, as I have often found in distilling water, turpentine, and other volatile oils, as well as spirits of wine and other liquids of low boiling-points. But if, when the charcoal is fully active, its function of absorbing vapour and liberating it under the action of heat be arrested and the charcoal itself transferred to a cold liquid, there is a condensation of vapour within its pores; so that in the second and some of the subsequent operations the spirit requires to be raised to the boiling-point before the vapour thus condensed can be made volatile and the charcoal become active. But if instead of an interval of five minutes the charcoal be left in the liquid during twenty-four hours, the condensation is more complete; and it requires a superior temperature to that of the boiling-point to vaporize the liquid within the pores of the charcoal. Thus we see that in the seventh and eighth operations the spirit rose 12° or 13° above its boiling-point before the charcoal nucleus became active; and its activity being thus restored, we see that in the next operation, after an interval of only five minutes, it became active at 148°, as before. It seems to me to be ignoring the obvious teaching of the facts before us to suppose, with M. Gernez, that the charcoal becomes inactive because the air has been driven out of it, when all that is wanted to restore its activity is a higher temperature; and this can be given either, as in the case before us, by the liquid becoming superheated, or by holding the retort over the flame of a spirit-lamp, which is the more expeditious way. Of course in the

practice of distillation fresh charcoal would be taken in each operation.

In the course of numerous operations with successive charges of wood-spirit and other liquids, I had frequent opportunities of noticing a fact which was pointed out in my paper read before the Royal Society*—namely, that if the thermometer contained in a liquid at, or near, or above the boiling-point be taken out, waved in the air, and then reinserted, it produces a fresh burst of vapour.   M. Gernez criticises this experiment, apparently without repeating it, so impressed is he that the thermometer thus exposed takes up a film of air, which, with him, is the only active nucleus.   But is not this a little contradictory?   He labours to prove that a clean surface cannot contract a film of air ; the thermometer in the above experiments presents a clean surface, and the momentary exposure is not sufficient to convert it into a dirty one.   But it is sufficient to allow the thermometer to pick up some particle of dust, a mote or a speck, which acts as a nucleus.   The thermometer used by me has its scale enclosed within a glass tube, the junction of which with the elongated bulb forms a somewhat deeply indented neck, which seems to be peculiarly favourable for receiving and retaining a nuclear speck ; so that in taking out the thermometer for the purpose of recharging the retort, it occasionally picked up a nucleus from the air ; and on restoring the thermometer to its place, the operation was sometimes disturbed by the activity of this nuclear point, from which issued an inverted cone of bubbles of vapour, often accompanied by a low musical note.   On some occasions the speck would be caught up by the bulb, or two or three specks at different parts of its surface, each originating a cone of bubbles.

In my paper in the 'Proceedings' I pointed out that the retort and other vessels used in the boiling and distillation of liquids frequently contain nuclear points which greatly assist the operation, and tend to save the vessel from destruction.   When the mode of heating is by means of a hot bath, these nuclear points appear to become active long before the liquid has attained the boiling-point.   Thus in heating wood-spirit, which boils at 148°, a nuclear point in the retort often becomes active at 90°.   The action of a bath on the belly of a retort of thin glass appears to be this :—The sides of the retort rapidly attain the temperature of the bath, which we may suppose to be at about 200°, while the liquid in the centre in contact with the bulb of the thermometer is only at 90°.   A thin layer of liquid is everywhere repelled from the inner surface of the belly of the retort and rises to the surface of the liquid, to be replaced by

* Proc. Roy. Soc. 1869, vol. xvii. p. 240.

another thin layer, which in like manner is also repelled ; and thus the temperature of the liquid rises rapidly by a kind of convection different from that which takes place when the source of heat is limited to the bottom of the vessel.   Each thin shell or layer thus repelled rises to the surface, flows over it, and forms a thin vapour-forming layer, not sinking immediately, or rather not being overlaid by the next rising layer of heated fluid until its temperature has been lowered by the giving off of vapour ; it then streams down as if it were colder and heavier : the surface which is nearly on a level with the surface of the liquid in the bath is, from the mode of applying the heat, somewhat lower in temperature than the rest of the liquid, while at such surface the vapour, however much the liquid be super-heated, tends to adjust itself to the true boiling-point of the liquid, although, as will be shown presently, the vapour may readily be superheated.

As to the highest point to which liquids can be superheated, there are certain statements which seem to me to belong to the region of the marvellous rather than to that of fair inference and common sense.   Thus Dufour dropped water into an oil-bath heated to 178° C. (352°·4 F.), and supposed that the water was of the same temperature as the bath because it remained in it without boiling ; and he points with apparent complacency to the fact that at that temperature the elastic force of aqueous vapour is equal to eight or nine atmospheres.   De Luc had already performed a similar experiment (the highest temperature of his oil-bath was 257° F.) ; but with commendable caution he remarks that he was by no means satisfied that the water ever reached that temperature.   " Ces gouttes d'eau, renfermées dans l'huile, pouvaient être dans un état particulier ;" by which he meant " the spheroidal state," although that term had not yet been given to a phenomenon which De Luc understood in its leading features.   The doubt expressed by him touches the weak side of most of the statements on superheating.   Phy-sicists have supposed that a liquid plunged into a hot bath must necessarily acquire the temperature of that bath.   Thus in Donny's celebrated experiment water contained in a tube 8 mil-lims. in diameter, and bent upwards in such a way as was supposed would prevent convection, was boiled in order to expel the air and sealed at the other end, which terminated in two bulbs. The end containing the water was then immersed during three minutes in three baths of calcic chloride in succession, the first at 113° C., the second at 121°, the third at 128° ; it was then transferred to a fourth bath at 132°, which rose to 138° ; but the tube had not been in this bath more than two and a half minutes when the water suddenly burst into steam which was

condensed in the two bulbs.   M. Donny remarks that this experiment is of a nature to show that the forces of cohesion and adhesion may play a very important part in the ebullition of water, the cohesion of water being thus supposed to be superior to a pressure of three atmospheres, or a column of water of 30 metres.

These are important conclusions, and ought not, as it seems to me, to rest on such data as the above experiment affords ; for I cannot help thinking that it is a false inference to conclude that because the tube containing the water was kept in the hot baths during eleven and a half minutes, the water necessarily acquired the temperature of the baths.   No evidence short of the actual reading of a thermometer in the water, as well as in the hot baths, ought to warrant so marvellous a statement as that water not under pressure (not even atmospheric in Donny's experiment) can be heated to 280^{c} F. without boiling.

In like manner, it seems to me that M. Gernez has been led into error by his mode of operating.   He had a thermometer in his hot bath (water or paraffine) with the bulb *near to* the tube containing the liquid ; and assuming that the liquid in the tube must be of the temperature of the bath, he states that such liquids as chloroform, wood-spirit, and bisulphide of carbon can support a temperature of 100° C. (212° F.) without boiling ; that is, that bisulphide of carbon can be raised 94° F., chloroform 70°, and wood-spirit 64° above their boiling-points without boiling, even when not under pressure.

Now, among the numerous experiments that I have tried with wood-spirit and other liquids in the only way that seems to me to lead to satisfactory results, namely with a thermometer in the liquid as well as in the hot bath, I have never seen the liquid rise to any thing like the temperature of the bath ; and how is it possible that it can do so, seeing that distillation is constantly going on from the surface of the liquid ; and although it may rise many degrees above its boiling-point, yet the tension of the pent-up vapour at length asserts itself long before the liquid has acquired the temperature of the bath.   Even in Donny's experiment with a tube not more than $\frac{3}{10}$ inch in diameter, the preliminary operation of boiling the water shows that evaporation from the surface must have been going on in the hot baths ; and this of itself was sufficient to keep down the temperature.

I requested my friend and former colleague, Mr. W. H. Hatcher, to conduct for me in his laboratory a series of experiments on the superheating of liquids, such as I could not readily carry on in a private house ; and I have to thank him and his assistant, Mr. C. Rumble, for the care and skill with which they have carried out my wishes.

The first arrangement was to boil distilled water in a flask briskly for some time in order to get rid of most of the air. A portion of this water was put into a test-tube dipping in an inclined position into a bath of oil in a copper bowl. A thermometer was placed in the water in the tube, and a second thermometer in the oil-bath. The tube and the inserted thermometer had been previously cleaned by means of nitric acid, potash solution, and copious rinsing in water.

Experiment 1.—Test-tube about $\frac{5}{8}$ inch in diameter inside. Barometer 30·275 inches.

Bath 22̊5	Water 21̊0. First bubble of steam.
240	214. Boiling in jerks.
260	218. Boiling.
280	218. „

Experiment 2.—Same tube. Barometer 30·245 inches.

Bath 225	Water 210. First bubble of steam.
240	214. Boiling violently.
290	216. Boiling briskly.

Experiment 3.—Tube about $1\frac{1}{4}$ inch in diameter. Barometer 30·245. The thermometer-bulb deep in the water, and nearly at the bottom of tube.

Bath 240	Water 210. First bubble of steam.
270	214. Boiling briskly; it
280	then remained quiet during three minutes, rising to
290	220. Quiet during four minutes.
310	222–224. Then a burst of steam, and temperature fell to 218°.
310	224. Burst of steam, and temperature fell to 220°.
310	224. Burst of steam, and temperature fell to 214°, and continued to boil.

Experiment 4.—Same tube. Barometer the same. Thermometer-bulb near the surface of the water.

Bath 270	Water 214. First bubble of steam, then quiet up to
	216. A few bubbles.
	218. Burst of vapour.
270	216. Boiling steadily for some minutes, then quiet.
285	222. Burst of vapour.
	219. Quiet, with evaporation from surface without bubbles until the bulb of the thermometer became uncovered.

The foregoing results agree with my own experience. The water becomes superheated a few degrees, then relieves the tension by a burst of vapour, and the temperature falls; there is also rapid but quiet evaporation from the surface, which also serves to keep down the temperature.

In asking my friend Mr. Hatcher to continue these experiments, I suggested the following particulars :—If the oil-bath were kept at a pretty steady high temperature, the results of the experiments would be more comparable. In the selection of tubes, avoid those that contain nuclear specks, which become evident when a liquid is boiled in a tube over the flame of a spirit-lamp by the stream of bubbles that escapes from one or two points. The results would also be more satisfactory if the tube were sustained in a vertical position in the bath. When inclined there is a shallow depth of liquid at the elongated elliptical surface, on which the heat acts more easily than on greater depths. The tube should not touch the bottom of the oil-bath ; for there the temperature is higher than just above. The thermometer in the tube must be quite clean. If the scale on the stem is filled in with black, this, being a mixture of lampblack and grease, is nuclear. The thermometer should not touch the bottom of the tube, because, if it does, a thin capillary film of liquid is formed beneath it, which readily bursts into vapour. If the rounded end of a glass rod or the thermometer-bulb be held against the bottom or side of the vessel containing a boiling liquid, a rapid stream of bubbles escapes from the point of contact. Nor should the thermometer-stem touch the side of the tube, for the same reason, but be suspended in its axis so as to be read easily without disturbing the liquid. Mechanical holders are useful in experiments of this kind.

The following results were obtained, subject, as nearly as possible, to the above conditions. The thermometer had an exterior casing tube. The tube used in boiling (except in one experiment) was about $1\frac{1}{2}$ inch in diameter ; and the thermometer was suspended freely within it with the bulb at about half the depth of the water. The difference in temperature between the bath and the water was maintained at about 60° F. until the water first boiled, when the temperature was allowed to rise.

Experiment 5.—A 1-inch tube used, but, being rather too narrow for the cased thermometer, was not employed again.

Bath 2̊75	Water 2̊08.	First bubble of steam.
	216.	Boiling briskly; a sudden burst of vapour caused the temperature to fall.
300	222.	Burst of vapour, and fall to
	216.	
300	224.	Burst, and fall to
	217.	
300	230.	Burst of vapour, and the whole of the water was blown out of the tube.

Experiment 6.—Wide tube of $1\frac{1}{2}$ inch in this and the next two experiments.

    Bath 315   Water 220. Burst of vapour.
                        216.
       to 325   222. Burst.
                        216.
    315 to 325   224. Burst.
                        216. Boiling steadily.

Experiment 7.—Ordinary Thames-water used; it was neither filtered nor boiled beforehand.

    Bath 300   Water 210. First bubble.
                        212. Boiling regularly; but after about twenty
                             minutes the boiling became irregular.
          310   217. Boiling regularly. There were particles
                             of flocculent matter in the water which
                             acted as nuclei.

Experiment 8.—With distilled water. A piece of cocoanut-shell charcoal was in the tube, care being taken not to touch it except with a clean glass rod. The charcoal had been previously boiled three times in water, and had been repeatedly placed under water in a nearly good vacuum. During the first exhaustion much air escaped from the charcoal; but in the last exhaustion it was difficult to get a bubble from it.

    Bath about 300.   Water 214. Boiling regularly; no jerks or
                             bursts of vapour until fully
                             half the water had been
                             boiled away, when the bulb
                             of the thermometer became
                             uncovered and the experi-
                             ment was stopped.

Experiment 9.—Spirits of wine, boiling at 170°, heated in test-tube about $\frac{3}{4}$ inch diameter in water-bath.

    Bath 190   Spirit 170. A constant stream of small bubbles
                             rising.
          190   174. Bubbles larger; the boiling more irre-
                             gular; it then ceased for two or three
                             minutes.
          200   184. Burst of vapour, and the temperature
                             fell.

Experiment 10.—Absolute alcohol. Boiling-point 176°.

    Bath 190   Alcohol 160. Minute bubbles rising.
          200   170. Bubbles larger, and all from one point
                             on stem of thermometer.
          204   178. Boiling constant from the nuclear point.

Experiment 11.—Absolute alcohol.

Bath 18$\overset{\circ}{4}$ Alcohol 15$\overset{\circ}{4}$. Minute bubbles as before.
195   170. Bubbles larger; an interval without any bubbles.
197   174. One large bubble.
         179. A large bubble rose at each rise of 1° up to
200   184.
         193. Burst of vapour.
         190. Burst.
200   182.
         187. Burst.
         176. The boiling now became regular.

Experiment 12.—Ether.   Boiling-point 95° F.

Bath 144   Ether 120. Evaporation regular, without bubbles, but so rapid that the bulb of thermometer soon became uncovered.

Experiment 13.

Bath 130   Ether 106. Burst of vapour, and a fall of temperature several times.

Experiment 14.—Ether in retort*.

Bath 210   Ether   ?. Boiling rapidly over.
178   120. Bursts of vapour from the bottom of the thermometer-bulb.
170   113.
166   111. Distillation proceeding rapidly. Globules in the spheroidal state moving rapidly over the surface.
160   112. Bursts of vapour.

Experiment 15.—Bisulphide of carbon in retort.   Boiling-point 118°·4 F.

Water-bath 186 Bisulphide 134. A point on the thermometer-bulb active, and liquid rapidly distilling.
180   130.
170   126. Point less active. Surface of liquid falling rapidly.
166   125. Nuclear point above the surface; liquid distilling.
160   123. No appearance of boiling.
155   121. Distilling.
148   120. Bulb uncovered.

Experiment 16.—In retort.

Bath 196 Bisulphide 120. Nuclear speck very active.
187   125. Distilling.
180   123. Speck active.
172   119.
165   118. Bulb uncovered.

* In this series the retort experiments were conducted by me.

Experiment **17.**—In retort.

Bath 20̊8	Bisulphide 13̊1.	Distilling from surface, but not boiling. Nuclear speck in the neck of the thermometer active.
198	136.	
190	136.	
185	136.	Nuclear point at the surface.
181	134.	Currents descending from the surface of the liquid in consequence of a portion of the vapour being condensed before it reached the tube of the retort.
177	131.	Nuclear point above the surface.
172	129.	Distilling rapidly.

The following experiments were made with a view to ascertain how far the vapour above the liquid would become superheated. For this purpose two thermometers were passed into a wide test-tube and so arranged that the bulb of one should dip into the liquid, while the bulb of the other should be just above its surface. Care was taken, as before, to have all the essential parts of the apparatus chemically clean.

Experiment **18.**—Absolute alcohol boiling at 176°.

Water-bath 208°.   Alcohol 186° 188° 190°.
                   Vapour 168   174   176.

During these observations there was a constant stream of bubbles forming apparently a continuous inverted cone of vapour ascending from a point on the stem of the immersed thermometer.

Bath 21̊2	Alcohol 19̊2. Vapour 176.	A burst of vapour with explosive force, and the alcohol fell to 190°; the vapour remained the same.
212	Alcohol 194. Vapour 177.	Another explosive burst of vapour, and a fall of 2° in the liquid.
212	Alcohol 192. Vapour 176.	A burst of vapour, and fall of 2° in the liquid.
212	Alcohol 190. Vapour 174.	Burst, and fall to 186°.

The bulb of the thermometer was now exposed. The inverted cone of bubbles ceased during the latter part of the observations.

Experiment **19.**—Wood-spirit boiling at 154°.

Bath 200°   Spirit 158° 161
            Vapour 148   151.

There was a constant stream of bubbles from a nuclear point on the bulb; it ceased spontaneously after a few minutes.

Bath 20̊5   Spirit 16̊7
            Vapour 154.

A stream of vapour set out again from the thermometer.

Bath 205°   Spirit 170. } Explosive burst, and the spirit fell to
             Vapour 158. }   168°.
     208   Spirit 172. } Explosive burst, and the spirit fell to
             Vapour 156. }   162°.

The thermometer-bulb now became uncovered.

Mr. Hatcher remarks on the above :—" In the intervals between the great bursts of vapour there were frequent and somewhat regular formations of large bubbles of vapour in the liquid, which seemed to heave up the whole surface at once.

" The thermometer in the vapour acted as a condenser, drops of condensed liquid constantly falling from the end of the bulb into the heated liquid below ; but in no case did the fall of one of these drops appear to be the cause of an explosion of vapour, or of one of the large bubbles.

" The thermometers were cleaned by being dipped into strong sulphuric acid, washed in a stream of water, dipped into potash solution, washed again, dipped into the liquid to be tested (this was contained in a separate vessel), and then transferred as quickly as possible to the test-tube in which the experiment was to be made. Even with these precautions we seem to have had nuclear points on the glass, although they ceased to act after a time.

" With respect to the apparent superheating of the vapour in the last experiment, I should note that the test-tube with the spirit stood vertically in a bowl of water, and was constantly surrounded by the steam from the water. It is quite possible that when the difference of normal temperature of the included vapour and the external steam is great (as in the case of the wood-spirit) some superheating of the included vapour may take place—not rom the superheated liquid below so much as from the steam-jacket enveloping the tube."

The following are some further experiments on the superheating of the vapour over the surface of the superheated liquid.

Experiment 20.—Ether and water-bath.

Bath 115°   Ether   96. } Burst of vapour.
             Vapour   88. }
     118   Ether   102. } Explosive burst of vapour.
             Vapour   92. }
     126   Ether   103. } 108
             Vapour   96. }   93
     132   Ether   112. }
             Vapour 101. }
     140   Ether   116. }
             Vapour 106. }

During these observations distillation was going on rapidly from the surface of the ether.

Experiment 21.

Bath 110° Ether 100. } Vapour 90. }
120 Ether 104. } Vapour 94. } First bubble of vapour.
124 Ether 110. } Vapour 94. }
140 Ether 114. } The vapour exploded and emptied the Vapour 96. } tube.

Experiment 22.

Bath 104 Ether 90. } Stream of small bubbles from the ther- Vapour 90. } mometer-bulb.
124 Ether 100. } Vapour 94. } Boiling general over the surface.
134 Ether 102. } Vapour 96. }
The bath gradually rose to 146 { Ether 96. } Vapour 120. } The boiling ceased.
140 Ether 122. } Vapour 108. }
144 Ether 123. } Vapour 110. } The immersed bulb became uncovered.

Experiment 23.—Water (distilled).

Bath 270 Water 216. } The bubbles arising from bulb, which had Vapour 168. } begun to rise earlier, now became larger.
270 Water 222. } A great upheaving of the water, and the Vapour 190. } temperature fell to 216°.
280 Water 222. } Explosive burst of vapour, and water fell Vapour 196. } to 218°.
280 Water 222. } Vapour 196. } Ditto, fell to 216°.
280 Do. Do.
280 Water 220. } Vapour 202. } Do.
300 Water 217. } Vapour 210. } Boiling became regular.

It appears from these experiments that ether-vapour readily becomes superheated from the column of vapour from the bath, as already stated, but that it is difficult (at least in this form of the experiment) to superheat a water-vapour. Mr. Hatcher says :—
" The tube in the water experiment must be surrounded by a certain column of air heated by the oil or paraffine-bath ; but the temperature (normal) of this vapour being high, it cannot take much heat from the surrounding column of air, and apparently its radiation of heat outwards for some time exceeds its absorption of heat inwards. The vapour during a part of the experiment is really underheated.

In conclusion, I venture to think, from a survey of the details

contained in this and my former paper, that, notwithstanding the elaborate attack of M. Gernez, the statements laid by me before the Royal Society in 1869 are confirmed.   It is proved, I think :—that there are such things as nuclei which act powerfully in separating vapour from water and other liquids at, or near, or above the boiling-point; that although air may assist the process of ebullition, air is not by any means the only nucleus, as M. Gernez affirms, but that various bodies which are not wetted by the liquid, but to which vapour will adhere, act as nuclei; that porous bodies such as charcoal, and especially the cocoanut-shell variety of it, are energetic in absorbing vapour, and discharging it under the action of the high temperature to which they are exposed; that the air of a room being full of particles of dust and motes, the momentary exposure of a clean surface is sufficient to enable it to pick up nuclei which act with energy in the boiling liquid; that the supposed inactivity of porous nuclei does not arise from their loss of air, but from the condensation of the vapours within their pores, seeing that they reassume their activity under the influence of a higher temperature than they had been before exposed to; that superheating is not an effect of a stable or permanent character; it is not, as has been supposed, the inverse effect of water cooled below its freezing-point and still retaining the liquid state, seeing that distillation is going on rapidly from the surface even though the mass of liquid be tranquil and many degrees above its boiling-point; that the temperature of the superheated liquid is always far below that of the bath, which is purposely raised much above the boiling-point of the liquid under examination; and that the statements respecting water and other liquids under ordinary pressure, or under no pressure at all, that they are capable of attaining a temperature in some cases more than double that of their boiling-points, is altogether a mistake, seeing that evaporation opposes a limit in one direction, and the increasing tension of the vapour a limit in another direction to the attainment of any such exalted temperature; that, in short, no statements as to the temperature of the superheated liquid can be correctly inferred from the temperature of the bath, the reading of a thermometer in the liquid itself being the only reliable evidence.

Highgate, N.   July 3, 1875

XIV. *On the Theorem of the Mean Ergal, and its Application to the Molecular Motions of Gases.* By R. CLAUSIUS.

§ 9. WE now turn to the second part of our investigation, namely the application of the theorem of the mean ergal to the molecular motions of gases.

In the consideration of gases, we start from the hypothesis that their molecules move in straight lines and only change their directions through collisions with one another or striking against the solid sides of the containing vessel. We will preliminarily regard the molecules as material points; that is, we will neglect the circumstance that the constituents of a molecule are also in motion relative to each other.

In reference to the mean length of path of the molecules between two collisions, I have already* arrived at certain results, some of which I will here briefly recapitulate.

After discussing the somewhat complicated action exerted by two moving molecules on approaching one another, I said that, for an approximate consideration of these occurrences, in which the question is only the determination of certain mean values, the notion of a *sphere of action* may be introduced; and this I defined as *a sphere described about the centre of gravity of the molecule, to the surface of which sphere the centre of gravity of another molecule can come before a recoil commences.*

If we picture to ourselves, as is sometimes done for the sake of clearness, the molecules as elastic balls, which on their surfaces meeting recoil from one another, we must conceive the diameter of the balls to be as great as the radius of the sphere of action just now defined. This radius I denoted by ρ. Further, the side of the little cube obtained when the molecules are imagined to be arranged cubically in the space which includes them I designated by λ, so that λ^3 signifies that portion of the space which falls to each molecule. With the aid of these symbols I constructed the following equation for the case in which only one molecule was in motion and the rest were fixed, the mean path-length of the moving molecule being l' :—

$$l' = \frac{\lambda^3}{\pi\rho^2}. \qquad \qquad (66)$$

When the other molecules are also moving, in order to determine the mean length of path, we need only diminish the last expression in the same ratio as the absolute velocity of the molecule considered is less than its relative velocity to the other mo-

* Pogg. *Ann.* vol. cv. p. 239; *Abhandlungensammlung*, vol. ii. p. 260; Phil. Mag. S. 4. vol. xvii. p. 81.

cules, when of course, the individual values being various, mean values must be taken.   By way of example, I calculated the case in which all the other molecules moved with exactly the same velocity as the molecule under consideration, and determined the mean length of path (in this case designated by l) by the equation

$$l = \tfrac{3}{4} \frac{\lambda^3}{\pi\rho^2}. \quad \cdots \quad \cdots \quad (67)$$

The meaning of this equation, which I further transformed into

$$\frac{l}{\rho} = \frac{\lambda^3}{\tfrac{4}{3}\pi\rho^3},$$

I expressed in the following proposition* :—*The mean length of path of a molecule is to the radius of the spheres of action as the entire space occupied by the gas is to that part of the space which is actually filled by the spheres of action of the molecules.*

If we imagine the molecules as elastic balls, we must (as already said) suppose the diameter of such a ball as great as the radius of the above-defined sphere of action, whence we get for the volume of the elastic ball one eighth of the volume of the sphere of action.   The last proposition then runs, *The mean length of path of a molecule is to one eighth of its diameter as the entire space occupied by the gas is to that part of it which is actually filled by the molecules.*

§ 10. To these previously found results on the collisions of the molecules we will connect our further considerations.   It will, however, be advisable not to employ the formulæ in their previous shape, but to regard the subject in a somewhat different manner, whereby we shall arrive at formulæ which are still more exact and have a form singularly suited to our present purpose.

Given a space bounded by any irregular surface.   At any place whatever within this space let there be a moving point, so that for all equally large portions of the space the probability of their containing the point may be equal.   Let the point make an indefinitely small movement in any direction whatever, so that all possible directions will be equally probable.   *Under these circumstances, what is the probability that the point with its indefinitely small movement, will strike the surface?*

We will first consider a single element ds of the surface, and inquire what is the probability that the point will strike precisely this element of the surface.

If dl is the indefinitely small extent of the point's motion, let us now imagine the point at rest, and, conversely, that the surface-element ds moves in the opposite direction about the portion dl.   Thereby an indefinitely small prismatic space will be de-

* Pogg. *Ann.* vol. cv. p. 250; *Abhandlungensammlung,* vol. ii. p. 272

scribed by the surface-element; and the probability that the point is situated exactly in this space is the same as the probability that the point in its motion strikes the surface-element *ds*.

For all cases in which the supposed motion of the surface-element is *outwards* from the enclosed space, so that the small space described by that element lies outside of the given space, the probability that the point will be in this small space is equal to zero. On the contrary, where the supposed motion of the surface-element is *inwards,* so that the small space described by it forms a part of the given space, the probability that the point will be found in exactly this part of the space is represented by a fraction whose numerator is this part, and its denominator the entire space.

If θ is the angle which the direction of the motion of the element makes with the normal erected inside upon the element, the magnitude of the small space is represented by the expression

$$\cos \theta ds\, dl,$$

which becomes positive or negative according as the small space is within or without the given space. Hence, if the entire given space be denoted by W, then with respect to the probability which is to be determined we can say :—For directions of motion with which the preceding expression becomes negative the probability is equal to zero ; and for those with which the expression becomes positive the probability is

$$= \frac{\cos \theta\, ds\, dl}{W}.$$

Now, in order to calculate the mean probability for all possible directions of motion, we must take account of the law of probability with respect to the angles. The likelihood that the angle which the direction of motion makes with the normal lies between a given value θ and the infinitesimally different value $\theta + d\theta$ is represented by the ratio of the area of a zone having the polar angle θ and the breadth $d\theta$ to the total surface of the sphere, therefore by the fraction

$$\frac{2\pi \sin \theta\, d\theta}{4\pi} = \frac{\sin \theta\, d\theta}{2}.$$

We have to multiply the foregoing fraction by this, and then to integrate for all values of θ for which $\cos \theta$ is positive, consequently from 0 to $\frac{\pi}{2}$. The probability that the point, if it traverses in any direction the distance *dl*, strikes in its motion the element of surface *ds*, will consequently be represented by

$$\int_{\theta=0}^{\theta=\frac{\pi}{2}} \frac{\cos \theta\, ds\, dl}{W} \cdot \frac{\sin \theta\, d\theta}{2} = \frac{ds\, dl}{2W} \int_{0}^{\frac{\pi}{2}} \sin \theta \cos \theta\, d\theta = \frac{ds\, dl}{4W}.$$

The same probability which holds for one surface-element, holds also for every other of equal magnitude. We get therefore at once for any finite portion *s* of the surface selected for consideration, or also for the entire surface (which shall be denoted by S), the following proposition :—*If within a space* W *bounded by the surface* S *a point, starting from any position whatever, traverses in any direction the infinitesimal distance* dl, *the probability that in its motion it will strike a certain part* s *of the surface is represented by*

$$\frac{s}{4W}\, dl,$$

and the probability that it will strike the surface generally is represented by

$$\frac{S}{4W}\, dl.$$

We will now assume that the point not merely traverses the infinitesimal distance *dl*, but, with a certain velocity *v*, continues to move until it strikes the surface and, according to the laws of elasticity, recoils, upon which its motion is continued with the same velocity in another direction. We will, besides, presuppose that the force exerted on the point by the surface acts only in its immediate vicinity, so that the alteration of the direction of motion at the collision takes place in an imperceptibly short time, and accordingly the velocity may be regarded as constant, notwithstanding the variation during the collision.

We can then, in the preceding proposition, replace the element of path *dl* by the product *vdt*, and say, *The probability that during the infinitesimal time* dt *the point will strike the surface is represented by*

$$\frac{Sv}{4W}\, dt.$$

Thence results, for the average number of collisions during unit time, which we will denote by P′, the equation

$$P'=\frac{Sv}{4W}\;;\quad . \quad . \quad . \quad . \quad . \quad (68)$$

and for the mean length of path, *l*′, we get, dividing *v* by P′, the equation

$$l'=\frac{4W}{S}.\quad . \quad . \quad . \quad . \quad . \quad (69)$$

§ 11. Let us now turn to the consideration of the molecules. Instead, however, of assuming all the molecules alike to be in motion, let us imagine a space in which very many molecules are in fixed positions—not, indeed, regularly arranged, but yet so far uniformly distributed that equal measurable sections of the

space contain equal numbers of molecules. Between these fixed molecules one molecule only shall move, which, now here, now there, shall strike against a fixed one and recoil from it. The problem is, to find the number of collisions during the unit of time; from which we shall then also obtain the mean length of path.

Instead of a moving molecule, we can consider a mere point in motion, if we at the same time imagine the above-defined spheres of action described about the centres of gravity of the fixed molecules, and assume that, as often as the moving point strikes against the surface of a sphere of action, it recoils from it. We thereby obtain again the case discussed in § 10, inasmuch as the surface of each sphere of action forms a part of the surface which limits the free space for the motion of the point. If the space in which the fixed molecules are found is circumscribed externally by a firm envelope, from which also the point recoils when it strikes it, then the surfaces of the spheres of action and the exterior bounding surface together constitute the entire surface which limits the free space for the motion.

We will first assume that the space furnished in the given manner with fixed molecules is infinitely extended, so that the moving point strikes against the surfaces of the spheres of action only, and not against a firm envelope. The number of the stationary molecules present may be determined by the statement that in volume V their number is N. The spheres of action of these N molecules occupy together the space $N\frac{4}{3}\pi\rho^3$; so that the part of the volume V remaining free for the motion of the point is represented by the difference $V - N\frac{4}{3}\pi\rho^3$. Further, the surfaces of the spheres of action of the N molecules form together a surface of the magnitude $N4\pi\rho^2$. The two expressions $V - N\frac{4}{3}\pi\rho^3$ and $N4\pi\rho^2$ we have to put in equations (68) and (69) in the places of W and S; we thus get

$$P' = \frac{N4\pi\rho^2 v}{4(V - N\frac{4}{3}\pi\rho^3)} = \frac{N\pi\rho^2 v}{V - N\frac{4}{3}\pi\rho^3}, \quad . \quad . \quad . \quad (70)$$

$$l' = \frac{V - N\frac{4}{3}\pi\rho^3}{N\pi\rho^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (71)$$

In order to compare this expression of l' with that in (66),

$$l' = \frac{\lambda^3}{\pi\rho^2},$$

we multiply numerator and denominator in the latter by N, and then put (corresponding with the signification of λ) $N\lambda^3 = V$, which produces

$$l' = \frac{V}{N\pi\rho^2}.$$

This expression differs from that given in (71) by this only—

that it has for its numerator the entire volume V, while the new one has only the part of the volume which is free for the motion as its numerator. The latter is more precise; but the difference is so slight that it may usually be neglected, because a characteristic peculiarity of gases is that only a very small part of the entire space occupied by a gas is actually filled by the spheres of action—especially if the gas is a *perfect* gas; for this idea itself implies that, compared with the entire space, the space filled by the spheres of action is a negligible quantity. After the foregoing, however, there is no difficulty in taking this slight difference in the formulæ into account; and in the above-mentioned proposition previously advanced we need only, instead of the words "the entire space occupied by the gas," to put "the part of the space which is left free from the spheres of action of the molecules."

If the space furnished with stationary molecules has only the magnitude V and is circumscribed by a firm envelope which the point strikes against and recoils from, and if we wish to take into consideration these collisions also, only a slight modification of the formulæ is necessary for this purpose. Let s be the magnitude of the outer circumscribing surface; we have then for S to put instead of $N4\pi\rho^2$ the sum $N4\pi\rho^2 + s$, by which we get

$$P' = \frac{(N4\pi\rho^2 + s)v}{4(V - N\frac{4}{3}\pi\rho^3)}, \quad \cdots \quad (72)$$

$$l' = 4\frac{V - N\frac{4}{3}\pi\rho^3}{N4\pi\rho^2 + s}. \quad \cdots \quad (73)$$

With gases not very much rarefied, however, the surface s is so small in comparison with $N4\pi\rho^2$ that this difference also can in most cases be neglected.

In the foregoing we have assumed only one molecule as moving and all the others as stationary. But in reality all the molecules are in motion; and it is inquired how in this case the number of collisions suffered by a molecule during the unit of time, and its mean length of path, can be determined.

If we again imagine first the space which is filled with moving molecules to be infinitely extended, so that the molecule considered strikes against other molecules only, and not against a firm envelope, in order to determine the number of collisions which it undergoes, we need only put in equation (70), in the place of the absolute velocity of the molecule, its mean relative velocity to all the other molecules; this we will denote by $\bar{r}$. We thereby get for the number of collisions (which in this case may be called P) the equation

$$P = \frac{N\pi\rho^2\bar{r}}{V - N\frac{4}{3}\pi\rho^3}. \quad \cdots \quad (74)$$

In order, further, to obtain the mean length of path, we must divide the velocity of the molecule by P; but as in this case its velocity does not remain invariable, but in general takes another value at each collision, we must employ its mean velocity $\bar{v}$. That gives, if the mean length of path is called l, the equation

$$l = \frac{V - N\frac{4}{3}\pi\rho^3}{N\pi\rho^2} \cdot \frac{\bar{v}}{\bar{r}}. \quad . \quad . \quad . \quad (75)$$

The value of the fraction $\dfrac{\bar{v}}{\bar{r}}$ here occurring depends on the law admitted in reference to the velocities of the molecules.

If the space in which the motion of the molecules takes place is bounded and surrounded by a firm envelope, and if, notwithstanding their small number, we will take into consideration the collisions of the molecule in question against the envelope, we must also take into account the circumstance that this molecule has not the same relative velocity to the envelope as to the other molecules: its mean relative velocity to the former is simply equal to its mean absolute velocity, consequently equal to $\bar{v}$. Accordingly equations (72) and (73), for the case of motion of all the molecules, change into

$$P = \frac{N4\pi\rho^2\bar{r} + s\bar{v}}{4(V - N\frac{4}{3}\pi\rho^3)}, \quad . \quad . \quad . \quad (76)$$

$$l = \frac{4(V - N\frac{4}{3}\pi\rho^3)\bar{v}}{N4\pi\rho^2\bar{r} + s\bar{v}}. \quad . \quad . \quad . \quad (77)$$

§ 12. We must now take up the question whether, and in what manner, the theorem of the mean ergal can be applied to the above-discussed molecular motions.

This proposition is applicable if the variables which determine the positions of the points change periodically, or if, at least for the single variables, time-intervals can be specified which, when used in forming the variations in the same manner as the periods of periodic variations, conduct to variations that satisfy the equation of condition occurring in the theorem of the mean ergal.

When from this point of view we contemplate the irregular motions of the molecules, we immediately perceive that no periodicity of the variables which determine the positions of the molecules is present. Even the fixing of the before-mentioned time-intervals appears, on consideration of the actual motion, to present considerable difficulties. I will therefore content myself for the present with stating another mode of treatment—less direct, it is true, but yet leading to reliable results. It is founded on the fact that the motion which really takes place can be replaced by another, in which all the quantities essential to our investigation have the same values as in the former, and

which satisfies the requisite condition for the applicability of the theorem of the mean ergal.

To facilitate contemplation, we will not immediately treat of the molecules in confused motion and striking against one another, but start from a simpler case. Given an infinitely extended space, furnished, in the manner spoken of in the last section, with stationary molecules, between which very numerous material points are circulating without affecting each other in their motions, but rebounding from the spheres of action of the molecules as often as they touch them.

The force suffered by a point from the action of the rebound we will conceive as proceeding from the surfaces of the spheres of action, and assume that only in the immediate vicinity of those surfaces does it become sensible, but then with still greater nearness increases very rapidly. In this case, as long as the point is at a sensible distance from all spheres of action, we may regard its *vis viva* as constant, and put its share in the ergal (which we will briefly call its ergal, since the motions of the individual points are independent of one another) equal to *nil*. But when the point comes close to the surface of a sphere of action, then the part of its *vis viva* referred to the direction normal to the surface very quickly diminishes to zero, and then, after the reversal of the normal component of its motion, just as quickly increases up to its original value. At the same time its ergal increases from zero by just the same amount as the *vis viva* diminishes, and then again sinks to zero. From this it follows that the *mean* ergal of each point must be very small in proportion to its mean *vis viva*, because only during a very short time has the ergal specifiable values—and, further, that with a given *vis viva* the mean ergal of a point must be proportional to the number of collisions it suffers during unit time.

For the number of collisions of one point, we have equation (70), namely

$$P' = \frac{N 4\pi\rho^2 v}{4(V - N\frac{4}{3}\pi\rho^3)}.$$

Let N' denote the number of moving points which are found simultaneously in a volume of magnitude V; and let us preliminarily assume that they all move with equal velocity v; then the total number of collisions which take place within the volume V during a unit of time will be represented by the product

$$N'P' = N' \frac{N 4\pi\rho^2 v}{4(V - N\frac{4}{3}\pi\rho^3)}.$$

The same number holds also for another case. Given a vessel in which a moving point only rebounds from the sides. In this

vessel let us imagine N′ material points which move with the common velocity v in all possible directions. Let the space-content of the vessel be called W, and its superficies S ; then we have, for the mean number of collisions of the single points during unit time, equation (68), viz.

$$P' = \frac{Sv}{4W},$$

and accordingly the total number of collisions taking place in the vessel during unit time will be represented by the product

$$N'P' = N' \frac{Sv}{4W}.$$

In order, therefore, to get the same number of collisions in this case as in the preceding, we have only to choose the vessel so that W and S are equal or proportional to the quantities $V - N\frac{4}{3}\pi\rho^3$ and $N4\pi\rho^2$.

Besides, the strokes against the surface of the sides of the vessel have the same multifariousness in respect of their directions as those against the surfaces of the spheres of action. If, finally, we assume that the force exerted on the points by the surface of the sides of the vessel is represented by the same function of the distance as the force which the surface of a sphere of action of a molecule exerts upon the points, then the augmentation and diminution of the ergal take place with the strokes against the sides just as with those against the spheres of action of the molecules. We must thus in both cases get the same mean ergal.

It can, however, be assumed that the velocities of the points moving among the stationary molecules vary according to some law, which law can be mathematically expressed by saying, "If one of the points be taken arbitrarily, the probability that its velocity lies between a given value v and the infinitesimally different value $v + dv$ is equal to $f(v)dv$," where f denotes a given function. In this case we must ascribe to the points which are in the vessel velocities corresponding to the same law ; and then the same mean *vis viva* and the same mean ergal are again obtained for the points in the vessel as for those which move between the stationary molecules.

As the above conditions which the space-content and the superficies of the vessel must satisfy do not determine its form, that of a rectangular parallelepiped can be chosen. If we then determine the positions of the points by rectangular coordinates parallel to the edges of the parallelepiped, we shall have, in these coordinates, variables which alter periodically.

We have accordingly arrived thus far—*to replace the very com-*

plicated motion of the points between the stationary molecules by another motion which has the same mean ergal and the same mean vis viva, *and at the same time is represented by periodic variations of the coordinates.*

§ 13. We now go further, to consider an infinitely extended space filled with molecules which are all in motion and strike against one another.

It has already been hinted that the number of shocks undergone by a single molecule under these circumstances can be obtained if we ascribe to the molecule in question its mean velocity relative to all the other molecules as its proper velocity, assuming that it moves among stationary molecules. From this we derive for the number equation (74), namely

$$\mathrm{P} = \frac{\mathrm{N}\pi\rho^2\bar{r}}{\mathrm{V} - \mathrm{N}\frac{4}{3}\pi\rho^3},$$

wherein N denotes the number of the molecules which are simultaneously present in a volume V*. In order, further, to deduce from this the total number of collisions which take place in the volume V during unit time, the last formula must be applied to all the N molecules. At the same time it must be recollected that each collision concerns not merely one molecule, but two, and thus occurs twice among those which all the individual molecules suffer. In order, therefore, to express the number of collisions which take place, we must not simply multiply the formula by N, but further divide the product by 2, and consequently obtain

$$\frac{\mathrm{NP}}{2} = \frac{\mathrm{N}^2\pi\rho^2\bar{r}}{2(\mathrm{V} - \mathrm{N}\frac{4}{3}\pi\rho^3)}.$$

We get the same number of collisions when we suppose that in a vessel of space-content $\mathrm{V} - \mathrm{N}\frac{4}{3}\pi\rho^3$, and superficies $\mathrm{N}4\pi\rho^2$; $\frac{1}{2}\mathrm{N}$ points are present and moving in all possible directions with velocities the mean value of which is $\bar{r}$, and, without reciprocal action on one another, rebound from the sides of the vessel.

As regards the force of the collisions, this depends on the angles of meeting and on the velocities. The various possible angles occur in the same manner with the collisions of the molecules as with the points striking against the sides of the vessel; and hence we can leave them out of view. With respect to the velocities, we remark, first, that with two molecules which strike

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* Strictly speaking, if N molecules are present in the selected volume V, each of the N molecules may strike against N−1 others: hence N−1 must be put in the above formula in place of N. But when V is a volume containing a ponderable quantity (*e. g.* a unit of weight) of the gas, N is a number so immensely great that 1 may be without hesitation neglected, and N put instead of N−1.

against each other the force of the shock is the same as if one of them were stationary and the other had the relative velocity r of the two molecules as its own velocity. Hence also a material point of the same mass as the molecule, if it strikes with the velocity r against a solid partition, will receive a shock of the same strength.

The relative velocities of the various occurring combinations of the molecules in twos necessarily differ; and their ratio can be mathematically expressed by saying that if out of all those combinations one be taken arbitrarily, the probability that the relative velocity of the two molecules belonging to it lies between a value r and the infinitesimally different value $r + dr$ is equal to $F(r)dr$, in which F denotes a function dependent on the law that holds for the velocities of the individual molecules. Assuming this function as given, we ascribe also to the $\frac{1}{2}N$ points in the vessel such velocities that, if one of the points be taken arbitrarily, the probability is equal to $F(r)dr$ that its velocity lies between r and $r + dr$.

In this case, not only is the total number of the shocks experienced by the points in the vessel equal to the total number of the collisions which take place between the molecules of the volume V, but there also occur exactly as many collisions of each degree of force in the vessel as among the molecules. Accordingly the $\frac{1}{2}N$ points in the vessel have the same mean ergal as the N molecules in the volume V.

Besides, it can be proved that also the *vis viva* of the $\frac{1}{2}N$ material points is equal to that of the N molecules. I have shown, in a recent memoir [*], that for any system of mass-points m_1, m_2, m_3, &c. in motion the following equation is valid—

$$\frac{1}{M} \Sigma m_\nu m_\mu r^2 = \Sigma m v^2 - M v_c^2 \quad . \quad . \quad . \quad . \quad (78)$$

where m_ν and m_μ are any two given masses, and the summation on the left side refers to all combinations of the given masses in twos, while that on the right-hand side is to be referred simply to all the masses. By M is understood the sum of all the masses, and by v_c the velocity of the centre of gravity of the entire system. When we apply this equation to the N molecules simultaneously present in the volume V, we have to put $v_c = 0$, and to assume that all the masses are equal to one another. From the latter circumstance it follows that we may put

$$M = N m,$$

and, if $\overline{v^2}$ denotes the arithmetic mean of all the values of v^2

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* "On various Forms of the Virial," Pogg. *Ann.* Jubelband, p. 411; Phil. Mag. S. 4. vol. xlviii. p. 1.

which occur,

$$\Sigma mv^2 = N m \overline{v^2}.$$

Further, m^2 can be substituted for the product $m_\nu m_\mu$; and since the number of the combinations of N masses in twos is equal to $\frac{1}{2}N(N-1)$, for which we can write $\frac{1}{2}N^2$ on account of the high value of N, if $\overline{r^2}$ denotes the arithmetic mean of all occurring values of r^2, we can put

$$\Sigma m_\nu m_\mu r^2 = \frac{1}{2} N^2 m^2 \overline{r^2}.$$

Inserting these values in equation (78) and dividing both sides by 2, we get

$$\frac{1}{2} N \frac{m}{2} \overline{r^2} = N \frac{m}{2} \overline{v^2}.$$

In this the equality of the *vires vivæ* is expressed; for the product $N \dfrac{m}{2} \overline{v^2}$ is the *vis viva* of the N molecules, and $\dfrac{1}{2} N \dfrac{m}{2} \overline{r^2}$ may be regarded as the *vis viva* of the $\frac{1}{2}N$ material points which have also the masses m, and with which the mean value of r^2 is the same as with the combinations of two molecules each.

We have consequently here also arrived at the same result as with the motion considered in the preceding section, viz. *that the $\frac{1}{2}N$ material points moving in the given manner in the vessel have the same mean ergal and the same mean* vis viva *as the N molecules flying through one another and striking against one another in the volume* V, *and that, so far as these quantities are concerned, the one motion can be replaced by the other.*

For the vessel we again choose a rectangular parallelepipedal form, by which the periodicity of the variations of the coordinates is attained. As, however, the expression $N4\pi\rho^2$, to which the superficies of the vessel must be equal, represents a very large surface for the space-content $V - N\frac{4}{3}\pi\rho^3$, we must assume that one side at least of the parallelepiped is very small. Call this side a. The ratio of the other two sides to each other we can select as we please; hence we will assume them to be equal, and denote each of them by b. Then the space-content of the parallelepiped will be ab^2, and its superficies $2b^2 + 4ab$; and putting these quantities equal to the two above mentioned, we obtain for the determination of a and b the equations :—

$$\left.\begin{aligned} ab^2 &= V - N\frac{4}{3}\pi\rho^3, \\ 2b^2 + 4ab &= N4\pi\rho^2. \end{aligned}\right\} \quad \ldots \ldots \quad (79)$$

For abbreviation, we will in these equations employ the symbol S (before used) for the total surface of all the spheres of

action, and at the same time denote by ϵ their total volume, putting

$$
\left.
\begin{aligned}
S &= N 4\pi\rho^2, \\
\epsilon &= N \frac{4}{3}\pi\rho^3.
\end{aligned}
\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (80)
$$

The equations will then be

$$
\left.
\begin{aligned}
ab^2 &= V - \epsilon, \\
2b^2 + 4ab &= S.
\end{aligned}
\right\} \quad . \quad . \quad . \quad . \quad . \quad (81)
$$

§ 14. We can now limit our further investigations to the case in which, in a rectangularly parallelepipedal vessel a vast number of material points are in motion, not influencing one another, but only subject to forces from the sides of the vessel causing them to rebound.

The force exerted on a point by a side of the vessel shall be perpendicular to the side, and represented by a function of the distance. If, then, we introduce rectangular coordinates parallel to the edges of the parallelepiped, the origin lying somewhere in its interior, x denoting one of the coordinates, and if further, of the two walls perpendicular to the direction of this coordinate, that situated on the positive side has the distance c, and that situated on the negative side the distance c' from the origin of coordinate, so that $c - x$ and $c' + x$ are the distances of the moving point from the two walls, we can represent the force which acts on the point along the x-direction by an expression of the form

$$
F'(c-x) - F'(c'+x),
$$

in which F' signifies a function which, we will assume, is very near equality to zero for all greater values of the argument, and only for very small values does its quantity become sensible, but then, on the argument still further diminishing, increases rapidly. Let F denote the integral of the function F'; the part of the ergal referring to the x-direction will, for the point under consideration, be expressed by

$$
F(c-x) + F(c'+x).
$$

The motions of the material points are to take place in all possible directions, and so that all possible directions are equally represented.

The velocities of the different points are to be different; and the law which prevails in this respect we will now express in the following manner. For this purpose we select the quantity $\frac{1}{2}\left(\frac{dx}{dt}\right)^2$ which we have already considered; and as in the course of the motion it is indeed constant during the greater portion of

the time, but during the small portions required for the collisions is variable, we take its mean value, which, as before, we will designate by w. This w has different values for different points; and hence, if $\mathfrak{w}$ denotes the arithmetic mean of all occurring values of w, we can put

$$w = \mathfrak{w}z^2, \quad \ldots \quad \ldots \quad (82)$$

wherein z is a quantity which, for the different points, may vary between 0 and ∞. We will refer the law to this quantity, representing the probability that for an arbitrarily selected point it lies between the value z and the infinitesimally different value $z + dz$ by $f(z)$, understanding by f a given function.

If, in correspondence with the assumptions made in the preceding section, the number of points present be denoted by $\frac{1}{2}N$, the law can also be expressed thus:—The number of the points for which that quantity lies between z and $z + dz$ is equal to $\frac{1}{2}Nf(z)dz$.

If the expression be integrated from $z = 0$ to $z = \infty$, the total number $\frac{1}{2}N$ must be obtained, whence it follows that the function f must satisfy the equation

$$\int_0^\infty f(z)\,dz = 1. \quad \ldots \quad \ldots \quad (83)$$

If for any quantity (preliminarily denoted generally by q) dependent on w, and which can therefore, when $\mathfrak{w}$ is given, be considered as a function of z, we wish to determine the arithmetic mean of all the values occurring, we have to multiply $\frac{1}{2}Nf(z)dz$ by q, integrate the expression from 0 to ∞, and divide the integral by $\frac{1}{2}N$. The mean value of q has consequently for its expression, since $\frac{1}{2}N$ is eliminated:—

$$\int_0^\infty qf(z)\,dz.$$

Applying this specially to the quantity w itself, for which $\mathfrak{w}z^2$ can be written, and recollecting that the mean value thus determined must be equal to $\mathfrak{w}$, we get a second condition-equation which the function f must satisfy—namely,

$$\int_0^\infty z^2 f(z)\,dz = 1. \quad \ldots \quad \ldots \quad (84)$$

Now, with respect to the form of the function f, Maxwell, as is well known, in his excellent "Illustrations of the Dynamical Theory of Gases "[*], deduced from the rules of probability a most important law for the velocities of the molecules. According to it, for an arbitrarily selected molecule, the probability that the

[*] Phil. Mag. S. 4. vol. xix. p. 19.

velocity-component referring to the x-direction lies between a value x' and an infinitesimally different value $x' + dx'$ is represented by the formula

$$\frac{1}{a\sqrt{\pi}}\, e^{-\frac{x'^2}{a^2}}\, dx',$$

wherein e is the base of the natural logarithms, and a a constant which depends on the vivacity of the motion, while x' may have any value from $-\infty$ to $+\infty$. An expression of the same form then holds also for the relative velocities of the different combinations of two molecules each. If we apply this law to the above-introduced quantity z, it reads:—The probability that the quantity denoted by z lies between the values z and $z + dz$ is represented by the formula

$$\sqrt{\frac{2}{\pi}}\, e^{-\frac{1}{2}z^2} dz,$$

wherein z may have any value from 0 to ∞. Accordingly, admitting this law, we obtain for the determination of the function f the equation

$$f(z) = \sqrt{\frac{2}{\pi}}\, e^{-\frac{1}{2}z^2}. \quad \ldots \quad (85)$$

We will now, for a single one of the moving points whose motions are independent of one another, form the equation expressing the theorem of the mean ergal; and we will keep to the form (III c.), which, if U provisionally signify the ergal of a single point, is applicable to this case, and reads:—

$$\delta\overline{U} = \frac{1}{2}\Sigma\overline{pq'}\,\delta\log u + \Sigma\overline{\frac{dU}{dc}}\,\delta c.$$

Herein, for our present purpose, the first sum on the right-hand side refers to the three coordinate-directions, and the second to the six boundary-planes of the parallelepiped.

But now, in the present case, not only are the motions of the different points independent of one another, but also, for each point, the components of the motion along the different coordinate-directions can be independently determined. Accordingly, with respect to one of the coordinate-directions (which we will again name the x-direction) we can form one of the preceding equations corresponding to it. Therein we must put

$$\frac{1}{2}\overline{pq'} = \frac{m}{2}\overline{\left(\frac{dx}{dt}\right)^2} = mw;$$

and the quantity u has the signification

$$u = m\left(\frac{dx}{dt}\right)^2 i^2. \quad \ldots \quad \ldots \quad (86)$$

I 2

Further, of the six boundary-planes, only those two come into consideration which are perpendicular to the x-direction. Lastly, we will, as before, denote the mean value of that part of the ergal which relates to the x-direction by mh, and also introduce the symbols g and g', their significations being determined by the equations

$$mg = \frac{\overline{d\mathrm{U}}}{dc}; \quad mg' = \frac{\overline{d\mathrm{U}}}{dc'}. \quad \cdots \quad (87)$$

In employing these symbols m can be omitted from all the terms, and the equation in question becomes

$$\delta h = w\delta \log u + g\delta c + g'\delta c', \quad \cdots \quad (88)$$

or, replacing w by $\mathfrak{w}z^2$,

$$\delta h = \mathfrak{w}z^2\delta \log u + g\delta c + g'\delta c'. \quad \cdots \quad (89)$$

This equation, in which any definite value is assumed for the quantity z, can, neglecting infinitesimal differences, be applied to all the points with which this quantity lies between the values z and $z + dz$, and the number of which is $\frac{1}{2}\mathrm{N}f(z)dz$; we here presuppose that the function f has the same form in the changed as in the original motion. The equation referred to these points may now be multiplied by $f(z)dz$ and integrated from $z=0$ to $x=\infty$; and it is to be remarked that the quantity z, which in the changed motion has the same infinite series of values as in the initial motion, with the variation indicated by the symbol δ is to be regarded as constant; and hence the factors z^2 and $f(z)dz$ may at pleasure be put before or after the variation-symbol, and in like manner the symbols of integration and variation may be interchanged. The new equation may therefore be written

$$\delta \int_0^\infty hf(z)dz = \mathfrak{w}\delta \int_0^\infty \log u . z^2 f(z)dz + \int_0^\infty gf(z)dz . \delta c$$

$$+ \int_0^\infty g'f(z)dz . \delta c'. \quad \cdots \quad \cdots \quad (90)$$

The integrals

$$\int_0^\infty hf(z)dz, \quad \int_0^\infty gf(z)dz \text{ and } \int_0^\infty g'f(z)dz,$$

according to what has been said above, denote the arithmetic means of all occurring values of h, g, and g', which means we will designate by $\bar{h}$, $\bar{g}$, and $\bar{g}'$. For a simpler notation of the fourth integral we will use the letter $\mathfrak{u}$, the meaning of which is determined by the equation

$$\log \mathfrak{u} = \int_0^\infty \log u . z^2 f(z)dz. \quad \cdots \quad (91)$$

Then the preceding equation reads:—

$$\delta \bar{h} = \mathfrak{w}\delta \log \mathfrak{u} + \bar{g}\delta c + \bar{g}'\delta c'. \quad . \quad . \quad . \quad . \quad (92)$$

We can construct an equation of this form for each of the three directions of coordinates. In doing so we put, for distinction, the indices 1, 2, and 3 to each of the quantities occurring in the equation, with the exception of $\mathfrak{w}$, to which we ascribe a common value for the three directions of the coordinates. If we add together the three equations thus produced, and multiply the sum by $\frac{1}{2}Nm$, we shall get

$$\tfrac{1}{2}Nm\delta(\bar{h}_1 + \bar{h}_2 + \bar{h}_3) = \tfrac{1}{2}Nm\mathfrak{w}\delta \log (\mathfrak{u}_1\mathfrak{u}_2\mathfrak{u}_3)$$

$$+ \tfrac{1}{2}Nm(\bar{g}_1\delta c_1 + \bar{g}'_1\delta c'_1 + \bar{g}_2\delta c_2 + \bar{g}'_2\delta c'_2 + \bar{g}_3\delta c_3 + \bar{g}'_3\delta c'_3). \quad (93)$$

But $\frac{1}{2}Nm(\bar{h}_1 + \bar{h}_2 + \bar{h}_3)$ is nothing else but the mean ergal of the entire system of points. If we now employ the letter U for the ergal relative to a definite time of the entire system, we should, properly, denote the *mean* ergal by $\bar{U}$; but as with a very great number of points which have different phases in their motions the value of the total ergal is always very nearly equal to its mean value, we can omit the stroke over the U. The product $\frac{1}{2}Nm\mathfrak{w}$ is one third of the total mean *vis viva*; we therefore designate it by $\frac{1}{3}T$, omitting here also the horizontal stroke over the T. As regards the quantities g_1, g'_2, g_3, &c., with the present signification of U and again omitting the stroke over its differential coefficients, the following equations are valid:—

$$\tfrac{1}{2}Nm\bar{g}_1 = \frac{\overline{dU}}{dc_1}; \quad \tfrac{1}{2}Nm\bar{g}'_1 = \frac{dU}{dc'_1}; \quad \tfrac{1}{2}Nm\bar{g}_2 = \frac{dU}{dc_2}, \quad \&c.$$

If we insert these values in equation (93) and collect the terms with the variations δc_1, $\delta c'_1$, δc_2, &c. under a sign of summation, we get

$$\delta U = \frac{1}{3}T\delta \log \mathfrak{u}_1\mathfrak{u}_2\mathfrak{u}_3) + \Sigma \frac{dU}{dc}\delta c. \quad . \quad . \quad (94)$$

[To be continued.]

XV. *On Unitation.*—IV. *The Unitates of Powers and Roots: developments of these, with applications.* By W. H. WALENN, *Mem. Phys. Soc.**

1. TAKING up unitation where it was left†, treating that which has been already put forward as introductory, and merely noticing that, if a number be expressed in terms of

its digits and of the powers of 10, the formula

$$(10-\delta)^{n-1}a + (10-\delta)^{n-2}b + (10-\delta)^{n-3}c + \ldots + (10-\delta)^2 s$$
$$+ (10-\delta)t + u$$

has the same remainder to δ as the number in question has (this being the fundamental formula of unitation), the following investigations in this paper refer to proofs and developments of the points advanced in the previous paper, in relation to the unitates of powers and roots.

2. If m and q be integers, $\sqrt[m]{q}$ irrational, and n any integer, excepting unity, so great that the expression $n^m - q$ is positive, then the unitate of the mth root of q to the base δ (or $U_\delta \sqrt[m]{q}$) is finite and integral when δ is of the form $n^m - q$. For if q be found in the recurring series that belongs to the unitates of the mth powers of numbers, $\sqrt[m]{q}$ will be found in the corresponding place in the series of the unitates of natural numbers, and this whether $\sqrt[m]{q}$ be rational or irrational. But for q to be found in such a series of unitates, it is necessary that δ should be of such a value that $r\delta$ ($r =$ an integer) being subtracted from any complete mth power gives q, or that

$$n^m - r\delta = q.$$

Now $U_\delta r\delta = \delta$; and this equation becomes

$$\delta = n^m - q.$$

If $n^m - q$ be less than q, the expression $U_\delta \sqrt[m]{q}$ will become $U_\delta \sqrt[m]{U_\delta q}$.

3. The following examples show some of the forms which $n^m - q$ may assume; at the same time they are proofs by induction of the general theorem.

Example I.—Find some of the values of δ, or some of the series of unitates, in which $\sqrt{2}$ has an integral unitate.

$$\delta = n^m - q = 2^2 - 2 = 2,$$

or

$$3^2 - 2 = 7,$$

or

$$4^2 - 2 = 14,$$

or

$$5^2 - 2 = 23,$$

or

$$6^2 - 2 = 34,$$

&c. &c.

Therefore the most useful system of unitation, for work in which $\sqrt{2}$ enters (δ being less than 10), is that which has its base equal to 7.

Example II.—Find some of the values of δ in which $\sqrt{3}$ has an integral unitate.

The values obtained from the formula $\delta = n^m - q$, by substitution of integral numbers for n, are 6, 13, 22, 33, 46, &c.

Example III.— $\sqrt{35}$ has integral unitates in the following systems—14, 29, 47, 65, 86, 109, &c., of which the first two refer to the expression $U_\delta \sqrt{U_\delta 35}$.

Example IV.—Integral unitates to the square root of 499 or of the unitate of 499 are found when the base of the system of unitation is one of the following numbers—30, 77, 126, 177, 230, 285, 342, 401, 462, 525, 590, 657, 726, &c.

Example V.—The unitation bases which give integral results in the case of $\sqrt[3]{47}$ are 17, 78, 169, 296, 475, 682, 953, 1284, &c.

4. Other values of δ may be found, which furnish integral values for the unitates of a given surd, in those systems which are exact divisors of the systems found by means of the formula $\delta = n^m - q$. For instance $\sqrt{3}$ has an integral unitate to the base 11, and 11 is an exact divisor of 22 and 33, both of which are found in the values of δ obtained from the formula $\delta = n^2 - 3$. Again, 23 is an exact divisor of 46, and accordingly the unitate $U_{23} \sqrt{3}$ is integral. This plan can always be relied upon; for it is evident that $\dfrac{n\delta}{n}$ will give the same unitate to a given number as δ.

5. Unitation squares may be used to obtain collateral proofs of the unitates of the roots of numbers. For instance, in the unitation square of the powers to the unitation base 7 in the order $a^{\frac{1}{8}}, a^{\frac{1}{4}}, a^{\frac{1}{2}}, a^1, a^2, a^4, a^8$, &c., the unitates that continue the law of series are $1^{\frac{1}{2}}, 2^{\frac{1}{2}}, 4^{\frac{1}{2}}$, and $7^{\frac{1}{2}}$. It would appear at first sight that, in the case of square roots, unitation squares give only one of the two roots; but it is not so; for in general $U_\delta a^{\frac{1}{2}} = U_\delta(+a^{\frac{1}{2}})$ or $U_\delta(-a^{\frac{1}{2}})$; that is to say, in the above instance, $U_7 \sqrt{2} = 4$ or $-4 = 4$ or 3 *.

6. The unitation square for the base 11, in the order $a^{\frac{1}{8}}, a^{\frac{1}{4}}, a^{\frac{1}{2}}, a^1, a^2, a^4, a^8, a^{16}$, &c., is given below; in the case of the square roots the whole-number unitates are comprised in the numbers 1, 3, 4, 5, 9, 11, and $-1, -3, -4, -5, -9, -11$, or 10, 8, 7, 6, 2, 11, the corresponding natural numbers being 1, 9, 5, 3, 4, 11. For instance, $U_{11} \sqrt{5} = 4$ or $-4 = 4$ or 7.

Unitates of Powers and Roots to base 11.

$a^{\frac{1}{8}}$.	$a^{\frac{1}{4}}$.	$a^{\frac{1}{2}}$.	a^1.	a^2.	a^4.	a^8.	a^{16}.	a^{32}.	a^{64}.
1	1	1	1	1	1	1	1	1	1
$2^{\frac{1}{8}}$	$2^{\frac{1}{4}}$	$2^{\frac{1}{2}}$	2	4	5	3	9	4	5
9	4	5	3	9	4	5	3	9	4
5	3	9	4	5	3	9	4	5	3
3	9	4	5	3	9	4	5	3	9
$6^{\frac{1}{8}}$	$6^{\frac{1}{4}}$	$6^{\frac{1}{2}}$	6	3	9	4	5	3	9
$7^{\frac{1}{8}}$	$7^{\frac{1}{4}}$	$7^{\frac{1}{2}}$	7	5	3	9	4	5	3
$8^{\frac{1}{8}}$	$8^{\frac{1}{4}}$	$8^{\frac{1}{2}}$	8	9	4	5	3	9	4
4	5	3	9	4	5	3	9	4	5
$10^{\frac{1}{8}}$	$10^{\frac{1}{4}}$	$10^{\frac{1}{2}}$	10	1	1	1	1	1	1
11	11	11	11	11	11	11	11	11	11

7. Unitation squares, however, will not always give all the whole-number unitates, on account of the law of the series being sometimes discontinuous, from abnormal causes. In the function $U_{17}a^n$, for instance, the values of the natural numbers in order being allotted to n, the repeating period is found to be 8 in the case of $U_{17}2^n$; this circumstance throws out the law of continuity in the series (to the base 17) 2^1, 2^2, 2^4, 2^8, &c., so that the 8th power and all higher terms have the unitate 1; the series of the unitates of natural powers are 2, 4, 8, 16, 15, 13, 9, 1, &c., and of the unitates of powers with common ratio 2 are 2, 4, 16, 1, 1, 1, &c. Nevertheless, on inspecting the series of squares belonging to the base 17 (namely 1, 4, 9, 16, 8, 2, 15, 13, 13, 15, 2, 8, 16, 9, 4, 1, 17), 2 is found to be the unitate of 6^2, therefore $U_{17}\sqrt{2}=6$ or 11; this result might have been expected from the fact that $34(=17 \times 2)$ is (in example I. of art. 3 in the present paper) found to be one of the values of δ in which $\sqrt{2}$ has an integral unitate.

8. The checking of calculations in which surds occur is therefore possible by unitation. To know which of the two unitates (in which the function $U_\delta \sqrt{a}$ occurs) is to be used, the following point, which applies to the bases 11, 9, and 7, may be profitably borne in mind. The only part of any formula in which a surd occurs, which can affect the unitate of the surd, is the factor by which it is multiplied; the presence of other additive or subtractive terms cannot affect the unitate in any way. That is, the form of a surd value being written $a + b\sqrt{c}$, a need not be noticed in reference to this question, and b is the sole quantity that influences the value of $U_\delta \sqrt{c}$. When $U_\delta b$ is odd, the least

value of $U_\delta \sqrt{c}$ is to be used; when $U_\delta b$ is even, the greatest value of $U_\delta \sqrt{c}$ is to be employed.

Those calculations in which surds merely enter as intermediate quantities, but disappear in the result, can readily be checked by using an appropriate system of unitation. The same facility occurs when surds remain throughout the calculation to the very end. Unitation has not yet been applied to the verification of the approximate decimal equivalents for surds.

9. The further examination of the question as to whether the function $U_{10^n}x$ can be made available for ascertaining the extreme right-hand figures of certain incommensurable quantities, presents considerable difficulties that are inherent in the nature of the subject. It seems to be within the bounds of correct interpretation to say that $U_{10}\sqrt{5}=5$, and that therefore 5 is the extreme right-hand figure of the decimal equivalent for $\sqrt{5}$; also that $U_{100}\sqrt[3]{29}=9$, and that therefore 9 is the terminal figure of that surd; also that $U_{100}\sqrt[3]{13}=17$, and that therefore 17 terminates $\sqrt[3]{13}$ when decimally expressed; but it appears difficult to get any reliable value to a greater number of figures than is contained in the natural number corresponding to the root. More extended experience in unitation may lead to more detailed results. In the mean time the use of the right-hand figures, when obtained, does not immediately appear.

10. In the first paper on unitation (Phil. Mag. Nov. 1868) a general statement was made of the theorem, and various examples were given. In the next paper (Phil. Mag. July 1873) the principles of inductive philosophy, and especially of successive induction, were brought to bear upon the ascertainment and explanation of negative and fractional unitates. In the third and last paper (Phil. Mag. May 1875) the position of unitation was examined in reference to its place among other operations. This classified treatment was found necessary to the proper working out of each division of the subject, considering the method of thought involved in each investigation. Interpretation of symbols has already been applied to the elucidation of the meaning of a negative unitate, and it can again be employed to show the meaning of $\sqrt{-1}$ in unitates.

The method of inquiry put forward in the Philosophical Magazine of July 1873, into the nature of a negative unitate, taking the number 9 as the value of δ, is capable of general statement, so that almost the same form of words may be used to prove the proposition that $U_\delta(-a)=U_\delta(\delta-a)$. It follows that a corresponding positive unitate may be found for every negative unitate. This method of working applies directly to the dealing with $\sqrt{-1}$ in unitation. Taking $a+b\sqrt{-c}$ as the type of the practical appearance of this quantity, the minus sign may be

eliminated by the above-mentioned principles ; for their application gives the equation $U_\delta(a + b\sqrt{-c}) = U_\delta(a + b\sqrt{\delta - c})$.

The further consideration of the meaning of $\sqrt{-1}$ in unitation is deferred until geometrical relations are explicitly treated of.

The peculiarities and practical uses of unitates are due in a great measure to their being periodic functions.

74 Brecknock Road, N.
    July 1875.

XVI. *On the Origin and Mechanism of production of the Prismatic (or columnar) Structure of Basalt.* By ROBERT MALLET, *F.R.S.*[*]

NO branch of physical geology is superior in interest or in the importance of its relations than that which regards the jointing, lamination, and cleavage of rock-masses.

Lamination and cleavage have been ably elucidated by Sharpe and others. Our knowledge of the mechanism of the jointing, though contributed to by Haughton, Phillips, and other observers, is still very far from complete. Least complete of all, however, is our knowledge of that which constitutes the most singular of all observed forms of jointing, namely that which has resulted in the production of that complicated system of joints evidenced in columnar basalt. Even before the controversy of the last century as to the igneous or aqueous origin of basalt was settled (and the conclusion admitted very generally that basalt is an igneous rock differing from other lavas chiefly in the conditions under which it was poured forth), the circumstances attending the production of such fissuring as ends in columnar structure was speculated on, but in the same superficial and inexact manner that has generally characterized the treatment of the subject by subsequent writers up to the present time. So far as the writer's acquaintance with the systematic works of British geologists and vulcanologists extends, including those of Bakewell, De la Beche, Lyell, Daubeny, Jukes, and several others, no consistent or even clearly intelligible theory of the production of columnar structure can be found. Even agreement upon the fundamental fact of

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[*] An abstract of the following paper was read to the Royal Society, and will be found in its Proceedings (No. 158), January 21, 1875. After five months had elapsed, and the paper, as I am informed, had been submitted to four referees in succession, I received notice that "it was not deemed expedient to print it at present." I offer no comment upon a decision as to the grounds for which I am necessarily ignorant ; but being unable to find any radical error vitiating the general views of my paper, I venture to publish it in its present form.

whether the prismatic jointing is to be attributed to the contraction by cooling of a previously melted mass cannot with certainty be gathered, or whether the structure is due to preexisting concretionary or crystalline arrangement of the integral particles of the mass, or to this co-acting with enormous external pressures, the origin of which is left perfectly vague, or to some play of successive and joint actions of all these various forces, the writer has found himself unable clearly to gather. Mr. Scrope, who has written somewhat at length on the subject ('Volcanoes,' second edit. p. 102, 1872), says, "It appears to me that the explanation of this columnar structure given in most text-books is scarcely complete or satisfactory. M. Delesse, indeed, holds it, as I do, to be the combined result of crystallization (rather I would say concretionary attraction) and contraction," *i. e.* by cooling; and it will be apparent upon collating chapter vi. pages 93–109, that the author considers the play of crystalline or concretionary forces, as they are vaguely termed, essential elements to the production of columnar structure (which is incapable of being produced by contractile forces alone), and does not view these concretionary or crystalline forces as merely modifying conditions acting subsequently to the production of columnar and jointed structure produced by contraction alone in the particles of the mass. The views here referred to differ but little from those expressed by Sir Henry De la Beche in his 'Geological Observer' of 1853, chap. xx.

Professor James Thompson, now of the University of Glasgow, communicated a paper to the Geological Section of the British Association in 1863 (an abstract of which appears in Brit. Assoc. Report, 1863, Proc. of Sections, p. 89), in which he develops some new views on this subject.

After the lapse of eleven years he has in substance repeated those views in a paper on the Giant's Causeway, read before the late Meeting of the British Association at Belfast (Belfast 'News Letter,' August 27, 1874). Professor Thompson has done service to science by correcting some erroneous statements as to matters of fact, and by showing that concretionary or crystalline forces have probably had nothing to do with the production of prismatic and jointed structure. He merely accepts the views of previous authors as to the production of the prisms by the contraction of the mass; and in that part of the paper which alone appears to the writer to possess claim to novelty, viz. his explanation of the production of the cup-formed cross joints, his own views appear to the writer untenable.

In the present paper the writer proposes to treat of the production of columnar and jointed structure in a somewhat more determinate manner than has, so far as his knowledge

goes, been hitherto done. He proposes to show that the play of contractile forces alone is sufficient, in a large mass of homogeneous material, when cooling from a state of igneous fusion, to account for all the observed phenomena of the production, by successive fissuring, of prismatic columns and of the cross joints or " cup-formed articulations " which separate these into longer or shorter pieces—and to show that concretionary or crystalline forces perform no necessary part in the process, and merely tend, through a greater or less want of homogeneity, more or less to modify the forms into which parts of the mass become separated. Incidentally it will be pointed out that the production of such jointing, or even of prismatic masses at all, by the mutual compression of superposed orbicular masses, in the way imagined by Gregory Watt, De la Beche, Scrope, and some others, is impossible and inconsistent alike with observed facts and with the physical conditions involved. The distinguishing characteristic of masses of prismatic basalt, when compared with the masses of lava-streams, are that the basaltic masses are tolerably regular in their general form, which is usually, and on the whole, more or less tabular, that the material before splitting up was, on the whole, very homogeneous throughout, and that there are few or no indications of joints or fissurings, whether curved or straight, except those separating the prisms themselves; whereas in great lava-streams the general forms of the whole mass are not regular nor tabular, and present great want of homogeneity. The masses, when large, have usually filled ravines or valleys, so that the lava-stream presents all the irregularities of contour of the valley which forms its mould—its upper surface being also highly irregular, and the interior of the mass, owing to the mechanism of its flow, being extremely heterogeneous. These conditions involve immense inequalities in the rate of cooling, and in the directions in which heat is principally lost at successive periods; hence internal strains through contraction and unequal subsidence result, which crack and divide it (by planes which are very generally curved) into masses of very various sizes and devoid of all regularity of form. The fractures, in fact, are often as irregular, and as much the result of what we may call accident, as are those into which a large lump of red-hot glass or red-hot flint becomes shattered by being thrown into water. To speak of such irregular breaking up as cuboidal, or as constituting one distinct and analyzable class of jointed structure in lavas, is merely to give rise to confusion.

It must be remarked, however, that this absolutely capricious fracturing may occasionally be seen passing into something like regularity and approach to columnar structure, where previously

unshattered lava-streams have cooled pretty symmetrically. Local and occasional admissions of surface-water through some of those cracks is one of the agents in their highly irregular distribution. These conditions may be well seen in some of the great lava-flows of Auvergne, which have been largely and deeply quarried for building-stones. There is therefore no analogy or true relationship between the capricious and accidental forces and conditions which have broken up the mass of lava-flows into blocks of the most diverse forms and dimensions, and those quite symmetric and determinable strains which have produced the splitting of basalt into jointed prisms. Yet instances occur in great lava-masses of the irregularly broken up and the prismatic structure in different parts of the same mass. The conditions producing this diversity need not be here enlarged upon.

The primary conditions to the production of straight prismatic structure in basalt are general homogeneity of the material, a tolerable regularity in the general form of the mass, and inequality in the rate of its cooling in one or more directions, *i. e.* from one or more of its bounding faces. The more or less horizontal and tabular masses of basalt as they occur in nature are usually cooled most rapidly from their upper or lower face— or if the basalt be found filling a dyke, then from the two opposite surfaces of the adjacent rock. Let us suppose a perfectly homogeneous, isotropic, and tabular mass of basalt resting upon a horizontal floor, all its dimensions being great and its length and breadth much greater than its depth. Let us suppose it cooled entirely from the upper surface, from every portion of which there is an equable loss of heat, such as would result from the superposition of a uniform mass of conducting material constantly maintained at a temperature inferior to that of the upper surface of the tabular mass of basalt. We have now to trace the progress of refrigeration and the progress and results of the internal strains due to its contraction by cooling. The mass, assumed as originally in liquid igneous fusion and nearly at the same temperature throughout, becomes less fluid, and finally rigid at the surface of cooling. Below this it is in a more or less plastic condition; and as the wave of heat passes outwards always parallel to the cooling surface, so the thickness of the rigid and of the viscous horizontal *couches* tend constantly to descend deeper into the mass of liquid basalt beneath. So long as nearly the entire mass remains liquid or viscous, the internal strains produced in it by the contraction due to cooling will be resolved into and met by a vertical descent of the whole mass and by internal movements in the still imperfect liquid. No splitting or fissuring can take place except in such upper laminæ of the mass

as have by cooling already arrived at such a degree of rigidity as no longer to permit of such movements.

The upper surface film first arrives at that temperature at which its rigidity becomes sufficient to prevent its yielding to the contractile strains in orthogonal directions and in the plane of its thickness in any way, except by its splitting up into much smaller surfaces which can draw off from contact with each other. We have assumed the mass isotropic—that is, contracting alike in all three dimensions for equal decrement of heat. The plates into which the rigid film tends to separate must therefore be symmetric and similar, and such as while in contact at their edges form the continuous film. What, then, determines the form of each of these separated plates? The tension between all the particles of a rigid film, such as we have supposed, contracting alike in orthogonal directions in reference to the plane of the film, can obviously be met by its subdivision in two directions only, namely by two sets of fissures transverse to any two horizontal axes—that is, by division of the film into square plates of a certain size; and this at first sight would seem to be the simplest mode in which the necessary relief to tension could be produced. It is not, however, the form into which the film actually does separate; for, as we see in nature, the normal form of the prisms of basalt is hexagonal, and the film which we are considering is but the elemental top or boundary of these prisms. What, then, determines the hexagonal rather than the square splitting up, or initiates the hexagonal rather than the square or other polygonal columnar structure? This important question seems to have been but insufficiently examined by previous writers.

There are but three forms all equal and similar to each other into one of which a surface can be divided leaving no vacancies, viz. the equilateral triangle, the square, and the regular hexagon, as in fig. 1; and into one or other of these, therefore,

FIG. 1
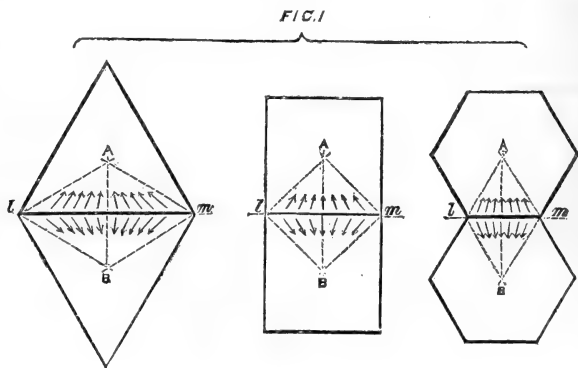
the surface must become divided—the first and last having their sides, singly or in pairs, respectively normal to three axes in the plane of the figure crossing each other at angles of 60°, and in the case of the square to two such axes crossing each other at 90°. What, then, in nature determines her choice of the complicated form of six sides in place of the simpler ones of three and four sides respectively? The reason is to be found in the "principle of least action," which, as a universal law, governs every operation of unbalanced force, the end to be obtained (to restore equilibrium) being effected in such a manner as to require the minimum expenditure of work. Now it can easily be shown that the smallest amount of work necessary to split up a given surface by contractile tensional forces within it will be that which divides it into hexagons rather than into equilateral triangles or squares, all being of equal area, and that whether the divisional figures be large or small in proportion to the surface.

In a mass contracting by cooling every particle tends to cause every other to approach nearer to it ; and if we take any two distant points a and b at random in such a body, all the particles round each point tend to approach it with forces proportional to some function of their respective distances from a and b. In a line drawn from a to b there are therefore equal and opposite tensional forces ; and when their amount exceeds the tensional resistance of the material, breach of continuity must occur somewhere across the line $a\,b$; and as every particle along that line is urged by equal and opposite forces through paths dependent upon the elasticity and coefficient of contraction of the material and proportional to its distance from a and b respectively, so the point of discontinuity must occur at the middle point between a and b ; and if the line have determinate width and breadth, *i. e.* be a small prism of the material between a and b, its fracture must occur at its mid length and normal to its axis. We are not concerned at present with determining the distance between a and b for a given material under the conditions of cooling at which breach of continuity must occur; but we will presently return to it. Let us apply the above reasoning to the rigid film forming the surface of the cooling mass of basalt already supposed. As every particle tends to draw every other particle towards it, the surface of the film, when breach of continuity occurs, must split up into figures of equal area ; for the tensional pull in all directions round points in the surface such as a and b is proportional to the number of particles, and to their distances round each, or to equal areas round each. Now the rending forces, or those tending to produce splitting up, are proportional to those areas ; and the resisting forces of the material are proportional to the perimeters separating these areas—that

is, to the lengths of the lines of fracture.   Referring now to the diagram (fig. 1), in which the three possible forms of subdivision are shown in two adjacent triangles, squares, and hexagons, the areas of all of which are equal, let rupture be supposed to take place between two adjacent areas along the lines of contact $l\,m$ of two adjacent figures, the rending forces proportionate to the areas may be supposed situated at the centre of gravity of A and B, and to act in the directions (along the line $l\,m$) of the short convergent arrows.   The resistance of the material to rupture is proportional to the perimeter of the figure ; or, as every side of every figure is opposed to a similar side of another adjacent figure, it is proportional to $l\,m$, or to a third of the total perimeter of the triangle, to one fourth of the perimeter of the square, and to one sixth that of the hexagon ; and the resistance to rupture along the lines $l\,m$ is in a direction perpendicular to the same.   Though the total rupturing force, being proportional to the area, is equal in all the figures, the effective rupturing force differs in all three, being dependent upon the relative obliquity with which the tensile forces between A and B act along the line $l\,m$—in other words, upon the integrals upon all possible angles of direction along $l\,m$ between the direct pull A B and the most oblique one l A m.   In other words, if f be the direct pull between A and B and θ be the angle at A between that line and that in the direction of any oblique force indicated by any one of the arrows, $f\cos\theta$ is the rending force in the latter direction. Integrating graphically and approximately the total rending effort along and perpendicular to the line $l\,m$ in the three respective figures by dividing the angle which it subtends in each respectively (viz. 120° in the triangle, 90° in the square, and 60° in the hexagon) into an equal number of equal parts, we find that the total effective effort to produce rupture along the line $l\,m$ in the three figures is proportional to the following numbers :—

> Equilateral triangle .   .   6·564
> Square   .   .   .   .   .   8·497
> Hexagon   .   .   .   .   . 10·261

or to the numbers 100, 129, 157.   The rending effort is therefore much the greatest in the hexagon.   But the resistance to rending is, as has been already shown, proportional in each figure to its perimeter or to the length $l\,m$ of one of its sides along which rupture occurs ; and the perimeters respectively for equal areas are as follows :—

> Equilateral triangle   .   .   45·6
> Square   .   .   .   .   .   .   40·0
> Hexagon .   .   .   .   .   .   37·224

or to the numbers 100, 87·7, 81·6. It thus appears that, amongst the three figures, the effort producing rupture is the greatest in the hexagon, while the resistance to rupture is the smallest; it is thus evident that the work done to produce fracture in the last-named figure will be much less than in either of the other two. Calling r the resistance to splitting in the respective cases and R the splitting or rending efforts, it is obvious that $\dfrac{r}{R}$ affords a measure of the relative work required for splitting, or

$$\frac{r}{R} = \frac{100}{100} = \text{unity for the triangle,}$$

$$\text{,,} = \frac{87·7}{129} = 0·68 \text{ for the square,}$$

$$\text{,,} = \frac{81·6}{157} = 0·519 \text{ for the hexagon,}$$

which is thus proved to require little more than one half the effort that would be necessary for division into equilateral triangles.

Splitting-up once commenced in this form at the cooling surface, must, as the cooled *couche* thickens, proceed deeper and deeper into the mass in a direction perpendicular to the surface of cooling, dividing the whole into hexagonal prisms, the lower terminations of the planes of fracture between which can at any moment never reach deeper into the mass than the distance at which it has cooled downwards from its original temperature to the " splitting-temperature," or to that at which the stiff viscous mass still existing deeper down shall have assumed sufficient rigidity to break when exposed to tension instead of yielding to it as a plastic mass. We have now to fix, as far as existing information will enable us, what is this superior limit of temperature at and below which splitting by contraction can commence and continue. And here, as in every other portion of the subject before us in which numerical data would be of the very highest importance, we have to lament their almost total absence. None of those who have speculated, more or less vaguely and in many words, upon the subject have ever thought it worth while to determine experimentally the dilatation by heat (*i. e.* the coefficient of contraction) of any one sort of basalt, or its extensibility at any temperature when exposed to tensile strain, or its total extension at rupture by such strains. Yet upon our possession of such data depends our being able to determine in actual measures the diameter of the prisms into which a given sort of basalt under given conditions shall split up. We

can therefore only state in general terms what are the circumstances which chiefly determine the size of the prisms, leaving to future observers the determination of the experimental data we need.   Good opportunities of observation of various metallurgic slags and the temperature at which actual lava ejections lose their plastic or viscous condition and assume sufficient rigidity to break by a steadily applied force (transverse or tensional), warrant the author's stating it to be between 900° and 600° F. in almost all basic silicates.   This setting temperature, as we may call it, is rather higher in the case of acid silicates, such as are most glasses ; and the temperature at which the rigidity is sufficient to admit of fracture by a blow or impulse is in all silicates a good deal higher than that appertaining to a steady strain.   A familiar example of this we see in sealing-wax, which at common temperatures behaves as a rigid body when broken by a blow, but behaves as a plastic one when exposed to a steady static strain.

Early in 1858 a smaller crater or bocca on Vesuvius frequently discharged, in volleys or singly, large irregular tails of viscous lava weighing often 300 or 400 pounds.   When these larger ones reached the surface of the mountain after their fall, they were generally too tough to be broken either by their fall or by the blow of a hammer ; they speedily cooled, however, first to a temperature at which they could be shattered by the hammer, and afterwards to one at which they could be broken across by a steady strain very inferior to that which would have broken the same mass when cooled to atmospheric temperature.   The superior limit of temperature at which the first stage of rigidity was reached was certainly below that of any "redness" visible in daylight but in complete shadow from the sun's rays.   It was therefore probably below 900° F. ; and the temperature of brittleness as to static strains appeared to the writer to be reached somewhat above 600° F., or about the melting-point of lead. We may estimate it, then, as probable that the splitting temperature in basalt may be reached somewhere between 900° and 600° F., and that while the material is above the superior of these two temperatures splitting by the contractile strains produced by slow cooling cannot occur, but that the effective contractile strains will be relieved by the stretching and distortion of the still plastic mass.   We have no actual data as to the amount of contraction for given decrement of temperature for basalt.   We may, however, approximate to it by reference to the author's experimental determination of the coefficient of total contraction between 3700° and 53° F. of iron furnace-slags having a chemical constitution closely resembling the mean of most basalts, as recorded in his paper " On the Nature and

Origin of Volcanic Heat and Energy " (Phil. Trans. Part 1, 1873), from which results he has since calculated partial mean coefficients for each 500°, or thereabouts, within the above limits. The partial mean coefficient for temperatures between 900° and 600° F. gives a lineal contraction = 0·00000396 per unit of length for 1° F. We may therefore provisionally conclude that basalt in cooling from the brittle point, or 600° F., may sustain a contraction for each 100 degrees of temperature lost of 0·000396 of a foot taken as unit, or of nearly 0·4 of a millimetre per metre in length. We know not at what amount of tensile strain per square inch of section basalt, within the limits of temperature here in question, will be torn asunder by mechanical tension, nor have we any information as to what is the amount of extension of the material at rupture, though we may be pretty certain that its tensile strength is considerable though probably inferior to that of glass, and its extensibility extremely small though probably greater than that of glass. It is certain, however, that whenever, upon a line as *a b* (*supra*) of whatever length, the amount of contraction by cooling through a given range of temperature exceeds the amount of extension upon the same length that would produce rupture, then splitting in one or more places between *a* and *b* must occur. It does not seem probable that the ultimate extension of basalt at rupture at any temperature below 600° would reach as much as 2 millims. per metre ; and if so, a reduction of 500° Fahr. in temperature would be more than sufficient to originate splitting. It is obvious also that the diameter of the prisms in any given instance depends mainly upon the numerical relations of these coefficients of contraction and of extensibility. The upper-surface film of the mass of basalt having once commenced to subdivide itself, as illustrated with exaggeration in fig. 2, the splitting proceeds downwards into the mass *pari passu* with its cooling as exhibited in fig. 3, and, at an after-stage,

FIG. 2
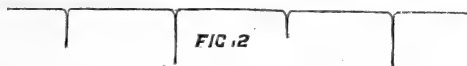
FIG. 3

in fig. 4. But the cooling of the mass no longer takes place from the top surface only as at the commencement ; refrigeration now takes place also from the faces (so far as they are opened

up) of every prism, so that each prism is in process of cooling from its sides as well as from the top, where it will be coldest,

FIG. 4

VISCOUS BASALT
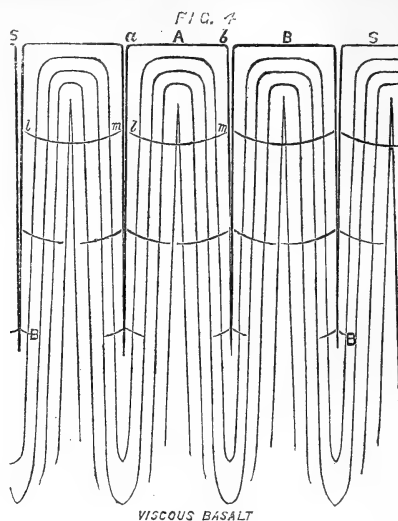
and hottest at the bottom where the mass is yet united but almost on the point of fracture. Thus the progress of cooling within this length of any one prism at a given instant may be indicated, as in fig. 4, by lines which are the vertical sections of isothermal *couches* or planes taken at successive intervals between the surface and the axis of the prism—which is of course hottest along the axis, and the exterior and succeeding *couches* of lowest temperature thickest at the upper part of the prism and thinnest at the lower part, where the time of cooling has been the shortest. We have now to consider the mode of production of the cross joints or articulated cup-like fractures observed in these prisms, and to show that they too are fully accounted for by the play of contractile forces. Before proceeding to this, it should be remarked that the cooling effects due to the progress of splitting asunder of the prisms, will be far more marked than might be at first supposed. Geological facts tend strongly to the conclusion that most basalts overflowed from fissures under water. Whether the liquid basalt flowed directly into contact with water or spread itself out laterally between a more solid bottom and a mass of loose matter resting upon it, or was poured out *sub dio* and exposed to rain &c., water would certainly find its way into all the joint-fissures opened between the prisms, and with an energy proportioned to the depth or head of water above it. Nor would the repulsion known to exist be-

tween highly heated bodies and liquids such as water, as shown by Boutigny's experiments, here come into play to resist the entrance of the water fully into the fissure; for, as we have seen, the splitting of the mass cannot commence except in those portions of it which have fallen to 600° Fahr. The temperatures of the sides of the prisms already split, and thus much below 600°, would therefore exercise no repulsion to the water which descended into these fissures, but would be for the most part at such temperatures as have been found the most favourable for the rapid communication of their heat to the water, and the production from it of steam at a pressure due to the superincumbent head. The conjoint action, therefore, of steam so produced demanding a vast supply for latent heat, and of rapid and circulatory movements given to the sheets of water within the fissures, would tend most powerfully to lower the temperature of the prisms, and that with an energy the greater as they became longer by the fissures extending more deeply into the mass.

It has been urged by some authors that water cannot have entered the fissures between the prisms, inasmuch as these are frequently found so close as not to admit the blade of a table-knife; but even if this closeness were universal, water would freely enter and ooze through such fissures at a moderate pressure. The actual water-pressure, however, to which many amygdaloidal basalts have been exposed must have been very great; for it has forced the sea-water and its saline contents in solution through the substance of the prisms as if through a filtering-stone, and so enabled the cavities of such basalt to become gradually filled with hydrous minerals, zeolites, chalcedonies, &c. Besides this, the thinnest film of water within the fissures must tend to widen these by hydrostatic pressure. It may also be remarked that an overflow of basalt covering a level surface must, by heating that surface, raise it slightly into a convex *umbo*, whence the fissures and prisms, when formed, must become slightly divergent; but after the whole has cooled completely, the umbo will disappear by contraction down to the original level surface, or even somewhat below it; and so all the prisms will approach each other towards their upper extremities, and thus in an after age be found to grip one another and be in closer proximity than at the time of their first formation. Returning now to the production of cross joints, we have assumed the tabular mass of basalt to be isotropic, its coefficient of contraction being thus the same in the vertical direction, or parallel to the axes of the prisms, as in the two others orthogonal thereto or horizontal. Were the temperature in any given section of a prism transverse to its axis the same throughout (*i. e.* the axial

portion of a prism no hotter than the exterior), then no reason
could be assigned why transverse fractures or cross joints should
be produced at all however long the prism; for every prism
being free and independent from its summit at S S (fig. 4) down
to B B (the lowest point at which splitting is actually taking
place*), the contraction of every prism lengthways would be
met simply by the shortening or subsidence of the prism itself.
But the exterior of every prism is colder than the interior por-
tion, the difference in temperature between the exterior and in-
terior portions being greatest at some point between B B and
S S (fig. 4). Differential strains in approximately the longitu-
dinal direction are therefore produced in each prism; so that
taking for origin the first cooled film S S (fig. 4), the outer por-
tions contracting most, tend to pull off or upwards or to approach
S S in the direction of the arrows as seen in fig. 5, and to leave
the central portions C behind for the time.

The effect of this inequality of strain is the
same in result as that which would be pro-
duced by some external force tending to
draw upwards or in one direction the ex-
ternal parts of the prism, and to leave
behind or relatively to push downwards
or in the opposite direction the central
parts thereof; and if the differential
strains be sufficiently powerful to origi-
nate transverse fracture, this must com-
mence all round the circumference of the
prism at some point of its length, and travel

FIG. 5

inwards until the whole prism becomes divided by a transverse
joint, which will happen when the central portions have cooled
down to the same temperature as that at which the external
portions began to divide transversely; and such joints will repeat
themselves at definite intervals along each prism, proceeding
downwards from the first or highest one, *l m*, fig. 4. We have
supposed our illustrative mass of basalt isotropic, contraction for
equal decrements of temperature being the same in all directions.
Under any given conditions as to the material and mode of cool-
ing, the first or uppermost transverse joint cannot occur nearer
the top surface S S, fig. 4, than the diameter from side to side of

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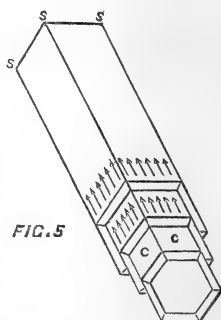
* In consequence of the elasticity of the material, the plane passing
through all the splitting-points at any instant will be found a little deeper
(by a distance which will be nearly constant) than, or in advance of the
isothermal *couche* to which the splitting is due (viz. 600° to 900° Fahr.),
just as a cleft produced by a wedge driven into an elastic block of wood
extends somewhat further than the point of the wedge itself; this remark
also applies to the transverse joints of the prism, hereafter referred to.

one of the prisms; for the conditions already stated that decide the diameters of the prisms, *a b*, fig. 4, equal to the distance between their centres A, B, decide also the length of line *a l*, in which the contractile strain becomes sufficient to fracture the material. The distance *a l* will actually somewhat exceed this, because the temperature of the prism at the place of the first joint is higher than at the surface S S; and the distance between the first and the second joint lower down will be somewhat greater than that of the first from the top; but after a certain length has been attained by the prisms, if their material were perfectly homogeneous and isotropic and the temperature of the unsplit mass the same at all depths, the distance between the joints would become constant, and never greatly exceed the diameter of the prism if that be large.

In nature the mass of basalt is never perfectly homogeneous or isotropic; and we shall hereafter consider the effects of the want of homogeneity, as well as of the diameter of the prisms, in disturbing the lengths between the joints.

XVII. *On two new Varieties of Vermiculites, with a revision of the other members of this Group*. *By* Josiah P. Cooke, *Jun.*, *Erving Professor of Chemistry and Mineralogy at Harvard College, and* F. A. Gooch, *Assistant in the Chemical Laboratory**.

SINCE the publication of the writer's first monograph on the vermiculites†, two new varieties of this group of minerals have been brought to his notice by Mr. W. W. Jefferis, of West Chester, Pa., who has most kindly furnished the materials for the following investigation.

The first of these varieties (which occurs at Lerni, Delaware Co., Pa.) has the following characters:—The unaltered mineral is of a dull sea-green colour, has a highly developed micaceous structure, is an aggregate of rough hexagonal plates, and of very imperfect external form. It is transparent in moderately thin laminæ, and is free from enclosed foreign matter. The optical characters of the mineral closely resemble those of the Culsagee variety of vermiculite, the angle between the optical axes varying in different parts of the same laminæ from 18° to 0°. Its hardness is about 1·5; and three determinations of its specific gravity (taken in alcohol at 23° C.) gave 2·409, 2·368, and 2·373.

Heated in a closed tube it gives off water acid in reaction, changes colour, and doubles its volume. Heated before the blowpipe it fuses to a dirty enamel.

The mineral was prepared for analysis by drying at 100° until its weight was constant, and in this condition was easily decomposed by hydrochloric acid. The bases, after the separation of silica, were converted into nitrates and separated by Deville's process. The iron and alumina were weighed together as oxides, and the magnesia as the pyrophosphate. The amount and condition of the iron were determined by decomposing the mineral by the process described by the writer in Silliman's American Journal, vol. xliv. p. 347, estimating by a standard solution of potassic permanganate the ferrous iron at once, and the total amount of iron after reduction to the ferrous state. Small amounts of lithium and potassium were found by the spectroscope in the residue obtained in Smith's process for the determination of alkalies; but the total amount of alkali probably did not exceed one tenth of 1 per cent. of the mineral. The mineral, dried at 100°, lost upon ignition

(1)	(2)	(3)	Mean.
11·65	11·67	11·71	11·68

per cent. of its weight. The details of these determinations will be referred to hereafter. Taking, then, 11·68 as the percentage of water in the mineral, the results of analysis are as follows :—

	(1)	(2)	(3)	Mean.	Ratio.
S̈i	38·17	37·93	38·00	38·03	2·54
Äl	12·84	13·07	12·87	12·93	·75
F̈e	7·02	7·02	7·02	7·02	·26
Ḟe	·50	·50	·50	·50	·01
Ṁg	——	29·72	29·57	29·64	1·48
L̇i, K̇	——	trace			
Ḣ	....	11·68	11·68	11·68	1·30
		99·92	99·64	99·80	

$$ \overset{..}{Si} : \overset{VI.}{R} + \overset{II.}{R} : \overset{.}{H} \qquad \overset{VI.}{R} : \overset{II.}{R} $$
$$ 2·54 : 2·50 : 1·30 \qquad 1·01 : 1·49 $$
$$ 2 : 2 : 1 \qquad 2 : 3 $$

The second of these two varieties of vermiculite occurs at Pelham, Mass. It has a greenish-yellow tint, resembling closely in colour the Culsagee variety. In the specimen examined, however, the scales were very much smaller than those of the Culsagee variety, and exhibited no regular outline. Examined with the microscope, they appeared equally free from interpene-

trating material. The plates do not show the striation observed both in Culsageeite and Jefferisite. No evidence of macling could be found. The plates were optically biaxial, with a small optical angle and strong negative double refraction like the other vermiculites and magnesian micas. The hardness of the mineral is about 1·5; two determinations of its specific gravity (taken in alcohol at 22° C.) gave 2·160 and 2·161. Heated in a closed tube it gives off water acid in reaction, changes colour, and doubles its volume, and in the forceps before the blowpipe fuses to a dirty enamel.

The mineral, dried at 100, lost upon ignition

(1)	(2)	(3)	(4)	Mean.
11·21	11·50	11·13	11·43	11·32

per cent. of its weight. The details of these determinations will be referred to hereafter.

In (1), (2), (3), and (4) of the following analyses the ignited mineral was decomposed by fusion with sodic carbonate; in (5) and (6) the mineral, dried at 100°, was decomposed by hydrochloric acid. In all, the ferric and aluminic oxides were weighed together, and the former subsequently determined by reduction and titration with a solution of potassic bichromate. In (4) the iron and alumina were together separated from magnesia by precipitation by ammonia with the usual precautions. In (5) the bases were converted into nitrates, and alumina and iron separated from magnesia by Deville's process. In both (4) and (5) the magnesia was weighed as the pyrophosphate. A trace only of ferrous iron was found. The spectroscope indicated small amounts of lithium and potassium*.

	(1)	(2)	(3)	(4)	(5)	(6)
$\ddot{\mathrm{S}}$i	41·14	41·28	41·04	40·78	41·27	41·23
$\ddot{\mathrm{F}}$e........	..	4·35	4·26	4·49	4·14	
$\ddot{\mathrm{A}}$l........	..			}15·05	15·19	
$\dot{\mathrm{M}}$g	..	}43·25	43·52	}28·82	28·25	
$\dot{\mathrm{L}}$i, $\ddot{\mathrm{K}}$	trace					
$\ddot{\mathrm{H}}$	11·32	11·32	11·32	11·32	11·32	
		100·20	100·14	100·46	100·17	

Analysis (5), which probably represents the amount of magnesia contained in the mineral more correctly than (4), affords

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* In Hallite and Jefferisite, as well as in the Lerni and Pelham varieties, small amounts of lithium were detected. In none of the vermiculites have we been able to detect fluorine.

the ratio

$$\overset{..}{Si} \ : \ \overset{...}{Al} \ : \ \overset{...}{Fe} \ : \ \overset{.}{Mg} \ : \ \overset{..}{H}$$
$$2\cdot75 \ : \ \cdot88 \ : \ \cdot17 \ : \ 1\cdot41 \ : \ 1\cdot26$$

$$2\cdot75 \ : \quad 1\cdot05 \quad : \ 1\cdot41 \ : \ 1\cdot26$$

or $\qquad 2\cdot75 \ : \qquad 2\cdot46 \qquad : \ 1\cdot26$

approximately $9 \quad : \qquad 8 \qquad : \ 4$

The writer has described (*loc. cit.*) the remarkable hygroscopic properties of the vermiculites, and the difficulty of separating the constitutional from the hygroscopic water which they may contain. The varieties from Lerni and Pelham offer the same difficulty in the determination of their water, thirty to forty hours being required to bring one or two grammes of either of them to a constant weight at 100° C.

In obtaining a constant temperature of 100° C., an electric regulator was used which differs from other similar forms of apparatus in simplicity of construction. The current is made or broken by a very slight rise or fall of mercury in a U-tube connected with a glass bulb within the air-bath. By means of a pressure-tap which closes an open L of the connecting tube, the air within the bulb may be confined as soon as the bath reaches the required temperature. After this a very slight increase of temperature raises the mercury column sufficiently to close the electric circuit, and then the current shuts the cock which regulates the supply of gas to the burner under the bath. The chief advantage and the novelty of the apparatus is to be found in the simplicity of this stopcock, which was suggested by Professor H. B. Hill. It consists of an ordinary chloride-of-calcium tube placed horizontally, and closed at the larger end by a rubber stopper which allows a considerable freedom of motion to a smaller glass tube passing through it; by this the illuminating gas enters the chloride-of-calcium tube, from which it passes to the burner. When the current is closed, an electromagnet acting on an armature attached to the outer end of the small tube plunges the curved inner end beneath the surface of some mercury in the bulb of the chloride-of-calcium tube and thus shuts off the main supply of gas, although a small orifice in the side of the inner tube allows a sufficient flow to keep the flame under the air-bath alive. The variation of the temperature of the air-bath does not ordinarily exceed one or two degrees during periods of fifteen to twenty hours, even under great variations of pressure in the gas-mains.

Table I. shows the percentages of water found in air-dried specimens of the Lerni vermiculite; and Table II. shows the percentages of water found in the same specimens dried at 100° C.

I.

	(1)	(2)
Loss at 100° C. . . .	5·75 ⎱	8·12
„ about 300° C.	10·97 ⎰	
„ red heat . .	..	8·60
	16·72	16·72

II.

	(1)	(2)	(3)	Mean.
Loss at about 300°.	11·65	2·54 ⎱ 9·13 ⎰	11·71	
„ red heat .				
	11·65	11·67	11·71	11·68

Table III. gives the percentages of water found in air-dried specimens of the Pelham vermiculite; and Table IV. gives the percentages of water found in the same specimens dried at 100° C.

III.

	(1)	(2)	(3)	(4)
Loss at 100° C. .	10·83	10·72	..	10·80
„ about 300° C.	4·35 ⎱	10·27	..	10·14
„ red heat . .	5·65 ⎰			
	20·83	20·99	..	20·94

IV.

	(1)	(2)	(3)	(4)	Mean.
Loss at about 300° .	4·90 ⎱	11·50	11·13	11·43	
„ red heat .	6·31 ⎰				
	11·21	11·50	11·13	11·43	11·32

The marked hygroscopic character of the Culsagee, Lerni, and Pelham vermiculites led to the idea that the discrepancies between the published analyses of Jefferisite and Hallite might be due to the hygroscopic nature of these minerals. The water contained in each of them was therefore carefully again determined. Table V. gives the percentages of water found in air-dried specimens of Jefferisite; analyses (1), (2), and (4) of Table VI. give the percentages of water found in the same specimens dried at 100°; (3 a) gives the percentages of water found in the mineral dried for three months over sulphuric acid, and (3 b) the percentages found in the same specimen subsequently dried at 100° C.

V.

	(1)	(2)	(3)	(4)
Loss at 100° C. . .	10·28	9·66	..	10·17
„ about 300° C. ⎱	9·58	⎰ 4·24 ⎱	..	9·36
„ red heat . . ⎰		⎱ 5·43 ⎰		
	19·86	19·33	..	19·53

VI.

	(1)	(2)	(3a)	(3b)	(4)
Loss at about 300° C. ⎱	10·47	⎰ 4·70	4·46	4·31 ⎱	10·42
„ red heat . . ⎰		⎱ 6·01	6·20	6·14 ⎰	
	10·47	10·71	10·66	10·45	10·42

The mean of (1), (2), (3*b*), and (4) of Table VI. is 10·51.

It would appear from analyses (3) that the amounts of water lost at 100° C., and over sulphuric acid during a period of three months, are very nearly identical. Assuming that 10·51 (the mean of the four determinations above) represents the percentage of water in the mineral dried at 100°, the following scheme contains the published analyses of Jefferisite reduced to this basis. Analyses (1), (2), and (3) are those of Professor Brush, Dr. Koenig, and Mr. Thomas M. Chatard respectively :—

	Si.	Äl.	Ḟe.	Fe.	Mg.	Ca.	K̇.	Ḧ.	Total.
(1)	38·47	18·24	10·92	1·31	20·38	·56	·45	10·51	100·84
	2·56	1·06	·34	·04	1·02	·02	·01	1·17	
Ratio			1·40		1·09				
	2·56	:	2·49			:		1·17	
(2)	37·25	19·87	8·17	2·36	21·51	—	—	10·51	99·67
	2·48	1·16	·31	·06	1·07			1·17	
Ratio			1·47		1·13				
	2·48	:	2·60			:		1·17	
(3)	38·04	18·39	8·84	2·33	21·34	—	—	10·51	99·45
	2·56	1·07	·33	·06	1·07			1·17	
Ratio			1·40		1·13				
	2·56	:	2·53			:		1·17	

Table VII. gives the percentages of water found in air-dried specimens of the green variety of Hallite ; and Table VIII. gives the percentages of water found in the same specimens dried at 100°. In analysis (4) the mineral was dried for three months

over sulphuric acid, and when subsequently heated to 100° for twelve hours met with no appreciable loss.

VII.

	(1)	(2)	(3)	(4)	Mean.
Loss at 100° . .	3·48	3·19	2·86	2·85	
,, about 300°. ⎫			2·03	2·28	
,, red heat . ⎬ 12·38	12·86	10·77	10·55	
	15·86	16·05	15·66	15·68	15·81

VIII.

Loss at about 300°. ⎫ 12·82	13·29	2·09	2·44	
,, red heat . ⎬		11·09	10·77	
	12·83	13·29	13·18	13·21

The mean of (2), (3), and (4), which agree closely, is 13·23. Reducing therefore Mr. C. E. Munroe's analysis of this same variety to this basis, the following scheme represents the constitution of the mineral dried at 100° :—

	S̈i.	Äl.	F̈e.	Ḟe.	Ṁg.	K̇.	Ḣ.	
	36·34	7·54	8·89	1·14	31·84	·47	13·23	99·45
Ratio	2·42	·44	·33	·03	1·59	·01	1·47	
			·77		1·63			
	2·42	:		2·40		:	1·47	

It will be noticed, however, that in the case of Hallite there appears to be a constant condition of hydration at about 300°, and that in two experiments the air-dried mineral lost above this temperature 10·77 and 10·55 per cent. of its weight. The mean of these values is 10·66; and regarding this as the water of crystallization of the mineral, and reducing Mr. Munroe's analyses accordingly, we obtain the following results :—

S̈i.	Äl.	F̈e.	Ḟe.	Ṁg.	K̇.	Ḣ.
37·17	7·72	9·06	1·18	32·57	·48	11·24
2·48	·45	·34	·03	1·62	·01	1·25
		·79		1·66		
2·48	:		2·45		:	1·25

It would appear, then, that Hallite at 300° is in the same condition of hydration which the other vermiculites examined assume at or about 100°. Now, corresponding to this, there is a very marked fact, indicated by the Tables given above, which is worthy of special notice. Air-dried Jefferisite loses at 100° about 10 per cent. of its weight, while air-dried Hallite loses

only about 3 per cent., showing that it holds its water much more firmly than the first. In order to institute a just comparison between the different vermiculites, it is obviously important to seek for each variety the point at which the mineral assumes a constant condition and maintains it through a considerable variation of temperature. Save only some practical convenience, there is no peculiar virtue in 100°, as the temperature at which a mineral should be dried for analysis. As in the case of crystalline salts, we should expect to find for each hydrous mineral a certain point or points of temperature at which it loses the whole or a part of its water of crystallization, and certain limits between which it maintains a constant composition. Moreover we should expect that these temperatures would be the more definite in proportion as what we may call the hygroscopic power is the more marked—that while in some cases the mineral would lose its water at a nearly constant temperature, and the intervals of definite hydration would be well marked, in others the loss would extend over a considerable range of temperature, and it would be more difficult to secure the states of definite composition. That such differences as these are conspicuous among the vermiculites the Tables given above abundantly show; but in addition to this evidence, the difference in the behaviour of the several varieties, when heated, impressed upon us more strongly the principle we have stated than the figures would indicate. Nevertheless, as the following Table shows, we have been able to bring all the vermiculites to essentially the same condition. The Table is merely a summing-up of the results already given, and exhibits a comparison of the atomic ratios* of the several varieties.

	IV. Si.	VI. II. R̶+R.	II. H̶.	VI. R̶.	II. R̶.
Hallite, at about 300° C.	2·49 : 2 :	2·47 : 2 :	1·25 1	·77 : 1 :	1·63 2
Pelham vermiculite, at 100° ..	2·75 : 2 :	2·46 : 2 :	1·23 1	1·05 : 2 :	1·43 3
Lerni ,, ,,	2·54 : 2 :	2·50 : 2 :	1·30 1	1·01 : 2 :	1·49 3
Culsagee ,, ,,	2·50 : 2 :	2·66 : 2 :	1·23 1	1·37 : 1 :	1·27 1
Millbury ,, (Crossley)	2·38 : 2 :	2·74 : 2 :	1·14 1	1·37 : 1 :	1·37 1
Jefferisite ,, at 100° ..	2·56 : 2 :	2·53 : 2 :	1·17 1	1·40 : 3 :	1·13 2

* The atomic ratio is the same ratio which in most works on mineralogy is still called the oxygen ratio. The numbers given in this paper are found by dividing the percentage of each oxide by a divisor which is the quotient of the molecular weight of the oxide divided by the quantivalence of the radical. See the writer's 'Chemical Philosophy,' p. 450, or paper "On Atomic Ratios," Amer. Journ. Sci. vol. xlvii. May 1869.

In the next Table we bring together some further noteworthy results, of which the details have already been given, indicating that in the case of three, at least, of the vermiculites we have evidence of different degrees of hydration corresponding to different temperatures :—

				IV.	VI. II.	II.
				Si.	R+R̈.	Ḧ.
Atomic ratio of Hallite air-dried			..	2·35 :	2·34 :	1·76 or 8 : 8 : 6
,,	,,	,,	at 100° ..	2·42 :	2·41 :	1·47 or 8 : 8 : 5
,,	,,	,,	at 300° ..	2·48 :	2·45 :	1·25 or 8 : 8 : 4
Atomic ratio of Pelhamite air-dried				2·45 :	2·19 :	2·32 or 4 : 4 : 4
,,	,,	,,	at 100° ..	2·75 :	2·46 :	1·26 or 4 : 4 : 2
,,	,,	,,	at 300° ..	2·90 :	2·59 :	·74 or 4 : 4 : 1
Atomic ratio of Jefferisite air-dried				2·30 :	2·28 :	2·17 or 4 : 4 : 4
,,	,,	,,	at 100° ..	2·56 :	2·53 :	1·17 or 4 : 4 : 2
,,	,,	,,	at 300° ..	2·68 :	2·65 :	·70 or 4 : 4 : 1

In the last two varieties the ratios correspond to the symbols

$$Si_2 \quad O_8 \quad (R, \ddot{R}) . 4H_2O$$
$$do. \qquad . 2H_2O$$
$$do. \qquad . H_2O.$$
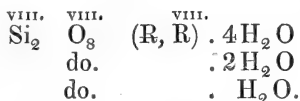
Here again it will be noticed that the differences in the definiteness of these ratios correspond to the differences of hygroscopic power described above. In the case of Hallite, the ratios are almost precisely those of even molecules, while in the case of Jefferisite the agreement is much less close.

In conclusion, we consider that the following general results may be deduced from this investigation :—

First. That all the vermiculites are unisilicates.

Secondly. That these minerals combine with water in several definite proportions, thus confirming the opinion advanced by the writer in his previous paper on this subject, that the water in the vermiculites is water of crystallization.

Thirdly. That all these minerals may be reduced to the condition expressed by the ratio 2 : 2 : 1, which we regard as the normal ratio of the vermiculites.

Fourthly. That the only essential difference between the different varieties of vermiculites is in the ratio between the sesquioxide and protoxide bases.

XVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 77.]

January 7, 1875.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following papers were read:—

"Remarks on a New Map of the Solar Spectrum." By J. Norman Lockyer, F.R.S.

I beg permission to lay before the Royal Society a portion of the new map of the solar spectrum referred to in one of my former communications.

It consists of the portion between w.l. 39 and 41.

I have found it necessary, in order to include all the lines visible in my photographs in such a manner that coincidences may be clearly shown, to construct it on four times the scale of Ångström's "Spectre Normal."

The spectra of the following elements have been photographed side by side with the solar spectrum and the coincidences shown:—

Fe, Co, Ni, Mn, Ce, U, Cr, Ba, Sr, Ca, K, Al.

The wave-lengths of new lines in the portion of this spectrum at present completed have been obtained from curves of graphical interpolation. Instead of the reading of a micrometer-scale, a photographic print of the spectrum has been employed in the construction of these curves, the wave-lengths of the principal lines being taken from an unpublished map of the ultra-violet region of the solar spectrum, a copy of which has been kindly placed at my disposal by M. Cornu. The photograph of the solar spectrum from the ultra-violet to beyond F, kindly given to me by Mr. Rutherfurd, has also proved of great service in the present work. I have, in fact, up to the present time, only been able to excel this photograph in the region about H.

From the extreme difficulty of carrying on eye-observations upon the portion of the spectrum now completed, Ångström's map is, of course, very incomplete about this region. The few lines mapped differ slightly in some cases from the positions assigned by Cornu; but the wave-lengths given by the latter observer generally fall into the curve without breaking its symmetry, and these positions have therefore been adopted. The advantage possessed by the photographic method over eye-observation may be estimated from the following numerical comparisons:—

Region of spectrum, 3900–4100.

Number of lines in Ångström's "Spectre Normal"			39
,,	,,	Ångström's and Thalén's map of the violet part of the solar spectrum	185
,,	,,	Cornu's map	205
,,	,,	new map	518

It will serve further to illustrate the advantages of the photographic method, to compare the number of lines in the spectra of metals already observed with the number of lines of the same metal given by Ångström in the " Spectre Normal."

Region of spectrum, 3900–4100.

Metal.	Lines in new map.		Lines in Thalén's map.
Fe	71		19
Mn	53		12
Co	47		—
Ni	17		—
Ce	163		—
U	18		—
Cr	24		—
Ba	7		—
Sr	5		—
Ca	7		6
K	2		—
Al	2		2
Total	416	Total	39

The purification of the various metallic spectra has at present been only partially effected; but I have seen enough already to convince me of the extreme rigour with which the principle I have already announced may be applied, while, at the same time, there are evidences that the application of it may lead to some results not anticipated in the first instance.

My object in laying these maps before the Society, and presenting this *ad interim* report of progress, is to appeal to some other man of science, if not in England, then in some other country, to come forward to aid in the work, which it is improbable that I, with my small observational means and limited time, can carry to a termination. I reckon that, having regard to routine solar work, it will require another year before the portion from H to G is completely finished, even for the metals the spectra of which are shown in the maps now exhibited. When this is done there will still remain outstanding all the ultra-violet portion, the portion from G to F (both capable of being photographed by short exposure), and the whole of the less-refrangible part (which both Draper and Rutherfurd have shown can be reached by long exposure with the present processes).

I cannot but think, moreover, that when the light which the spectroscope has already thrown upon molecular action shall be better known, and used as a basis for further inquiry, methods of photography greatly exceeding the present one in rapidity, in the less-refrangible portion of the spectrum, will be developed and utilized in the research.

The map is being drawn by my assistant, Mr. Raphael Meldola (to whom my thanks are due for the skill and patience he has

brought to 'bear upon the work), in the first instance, with more especial reference to the positions, thicknesses, and individualities of the lines; the final revision will consist of an absolute intensity reproduction of the photographs.

"On Acoustic Reversibility." By J. Tyndall, D.C.L., LL.D., F.R.S.

On the 21st and 22nd of June, 1822, a Commission appointed by the Bureau of Longitudes of France executed a celebrated series of experiments on the velocity of sound. Two stations had been chosen, the one at Villejuif, the other at Montlhéry, both lying south of Paris, and 11·6 miles distant from each other. Prony, Mathieu, and Arago were the observers at Villejuif, while Humboldt, Bouvard, and Gay-Lussac were at Montlhéry. Guns, charged sometimes with 2 lbs. and sometimes with 3 lbs. of powder, were fired at both stations, and the velocity was deduced from the interval between the appearance of the flash and the arrival of the sound.

On this memorable occasion an observation was made which, as far as I know, has remained a scientific enigma to the present hour. It was noticed that while every report of the cannon fired at Montlhéry was heard with the greatest distinctness at Villejuif, by far the greater number of the reports from Villejuif failed to reach Montlhéry. Had wind existed, and had it blown from Montlhéry to Villejuif, it would have been recognized as the cause of the observed difference; but the air at the time was calm, the slight motion of translation actually existing being from Villejuif towards Montlhéry, or against the direction in which the sound was best heard.

So marked was the difference in transmissive power between the two directions, that on the 22nd of June, while every shot fired at Montlhéry was heard " à merveille " at Villejuif, but one shot out of twelve fired at Villejuif was heard, and that feebly, at the other station.

With the caution which characterized him on other occasions, and which has been referred to admiringly by Faraday[*], Arago made no attempt to explain this anomaly. His words are :—" Quant aux différences si remarquables d'intensité que le bruit du canon a toujours présentés suivant qu'il se propageaient du nord au sud entre Villejuif et Montlhéry, ou du sud au nord entre cette seconde station et la première ; nous ne chercherons pas aujourd'hui à l'expliquer, parce que nous ne pourrions offrir au lecteur que des conjectures denuées de preuves"[†].

I have tried, after much perplexity of thought, to bring this subject within the range of experiment, and have now to submit to the Royal Society a possible solution of the enigma. The first step was to ascertain whether the sensitive flame referred to in my recent paper in the Philosophical Transactions could be safely employed in experiments on the mutual reversibility of a source of sound and an object on which the sound impinges.

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[*] Researches in Chemistry and Physics, p. 484.
[†] Connaissance des Temps, 1825, p. 370.

Now the sensitive flame usually employed by me measures from 18 to 24 inches in height, while the reed employed as a source of sound is less than a square quarter of an inch in area. If, therefore, the whole flame, or the pipe which fed it, were sensitive to sonorous vibrations, strict experiments on reversibility with the reed and flame might be difficult, if not impossible. Hence my desire to learn whether the seat of sensitiveness was so localized in the flame as to render the contemplated interchange of flame and reed permissible.

The flame being placed behind a cardboard screen, the shank of a funnel passed through a hole in the cardboard was directed upon the middle of the flame. The sound-waves issuing from the vibrating reed placed within the funnel produced no sensible effect upon the flame. Shifting the funnel so as to direct its shank upon the root of the flame, the action was violent.

To augment the precision of the experiment, the funnel was connected with a glass tube 3 feet long and half an inch in diameter, the object being to weaken by distance the effect of the waves diffracted round the edge of the funnel, and to permit those only which passed through the glass tube to act upon the flame.

Presenting the end of the tube to the orifice of the burner (*b*, fig. 1), or the orifice to the end of the tube, the flame was violently agitated by the sounding-reed, R. On shifting the tube, or the burner, so as to concentrate the sound on a portion of

Fig. 1.
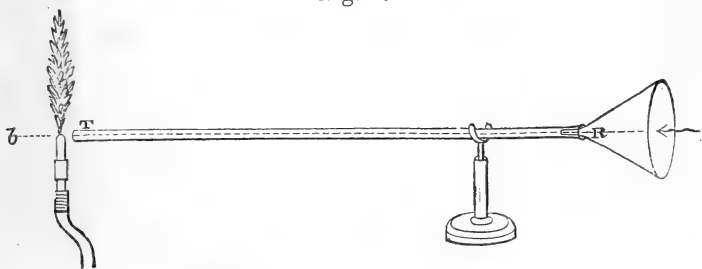
the flame about half an inch above the orifice, the action was *nil*. Concentrating the sound upon the burner itself about half an inch below its orifice, there was no action.

These experiments demonstrate the localization of "the seat of sensitiveness," and they prove the flame to be an appropriate instrument for the contemplated experiments on reversibility.

The experiments proceeded thus :—The sensitive flame being placed close behind a screen of cardboard 18 inches high by 12 inches wide, a vibrating reed, standing at the same height as the root of the flame, was placed at a distance of 6 feet on the other side of the screen. The sound of the reed, in this position, produced a strong agitation of the flame.

The whole upper half of the flame was here visible from the

reed ; hence the necessity of the foregoing experiments to prove the action of the sound on the upper portion of the flame to be *nil*, and that the waves had really to bend round the edge of the screen so as to reach the seat of sensitiveness in the neighbourhood of the burner.

The positions of the flame and reed were reversed, the latter being now close behind the screen, and the former at a distance of 6 feet from it. The sonorous vibrations were without sensible action upon the flame.

The experiment was repeated and varied in many ways. Screens of various sizes were employed ; and instead of reversing the positions of the flame and reed, the screen was moved so as to bring, in some experiments the flame, and in other experiments the reed, close behind it. Care was also taken that no reflected sound from the walls or ceiling of the laboratory, or from the body of the experimenter, should have any thing to do with the effect. In all cases it was shown that the sound was effective when the reed was at a distance from the screen and the flame close behind it ; while the action was insensible when these positions were reversed.

Thus, let *s e*, fig. 2, be a vertical section of the screen. When the reed was at A and the flame at B there was no action ; when the

Fig. 2.

reed was at B and the flame at A the action was decided. It may be added that the vibrations communicated to the screen itself, and from it to the air beyond it, were without effect ; for when the reed, which at B is effectual, was shifted to C, where its action on the screen was greatly augmented, it ceased to have any action on the flame at A.

We are now, I think, prepared to consider the failure of reversibility in the larger experiments of 1822. Happily an incidental observation of great significance comes here to our aid. It was observed and recorded at the time that while the reports of the guns at Villejuif were without echoes, a roll of echoes, lasting from 20 to 25 seconds, accompanied every shot at Montlhéry, being heard by the observers there. Arago, the writer of the Report, referred these echoes to reflection from the clouds, an explanation which I think we are entitled to regard as problematical. The report says that " tous les coups tirés à Montlhéry y étaient *accompagnés* d'un roulement semblable à celui du tonnerre." I have italicized a very significant word—a word which fairly applies to our experiments on gun-sounds at the South Foreland, where there was no sensible

solution of continuity between explosion and echo, but which could hardly apply to echoes coming from the clouds. For supposing the clouds to be only a mile distant, the sound and its echo would have been separated by an interval of nearly ten seconds. But there is no mention of any interval; and had such existed, surely the word "followed," instead of "accompanied," would have been the one employed. The echoes, moreover, appear to have been *continuous*, while the clouds observed seem to have been *separate*. "Ces phénomènes," says Arago, "n'ont jamais eu lieu qu'au moment de l'apparition de quelques nuages." But from separate clouds a continuous roll of echoes could hardly come. When to this is added the experimental fact that clouds far denser than any ever formed in the atmosphere are demonstrably incapable of sensibly reflecting sound, while cloudless air, which Arago pronounced echoless, has been proved capable of powerfully reflecting it, I think we have strong reason to question the hypothesis of the illustrious French philosopher.

And considering the hundreds of shots fired at the South Foreland, with the attention specially directed to the aerial echoes, when no single case occurred in which echoes of measurable duration did not accompany the report of the gun, I think Arago's statement that at Villejuif no echoes were heard when the sky was clear must simply mean that they vanished with great rapidity. Unless the attention were specially directed to the point, a slight prolongation of the cannon-sound might well escape observation; and it would be all the more likely to do so if the echoes were so loud and prompt as to form apparently part and parcel of the direct sound.

I should be very loth to transgress here the limits of fair criticism, or to throw doubt, without good reason, on the recorded observations of an eminent man. Still, taking into account what has been just stated, and remembering that the minds of Arago and his colleagues were occupied by a totally different problem (that the echoes were an incident rather than an object of observation), I think we may justly consider the sound which he called "instantaneous" as one whose aerial echoes did not differentiate themselves from the direct sound by any noticeable fall of intensity, and which rapidly died into silence.

Turning now to the observations at Montlhéry, we are struck by the extraordinary duration of the echoes heard at that station. At the South Foreland the charge habitually fired was equal to the largest of those employed by the French philosophers; but on no occasion did the gun sounds produce echoes approaching to 20 or 25 seconds' duration. It rarely reached half this amount. Even the syren-echoes, which were more remarkable and more long-continued than those of the gun, never reached the duration of the Montlhéry echoes. The nearest approach to it was on the 17th of October, 1873, when the syren-echoes required 15 seconds to subside into silence.

On this same day, moreover (and this is a point of marked

significance), the transmitted sound reached its maximum range, the gun sounds being heard at the Quenocs buoy, which is $16\frac{1}{2}$ nautical miles from the South Foreland. I have already stated that the duration of the air-echoes indicates "the atmospheric depths" from which they come[*]. An optical analogy may help us here. Let light fall upon chalk, the light is wholly scattered by the superficial particles; let the chalk be powdered and mixed with water, light reaches the observer from a far greater depth of the turbid liquid. The solid chalk typifies the action of exceedingly dense acoustic clouds; the chalk and water that of clouds of moderate density. In the one case we have echoes of short, in the other echoes of long duration. These considerations prepare us for the inference that Montlhéry, on the occasion referred to, must have been surrounded by a highly diacoustic atmosphere; while the shortness of the echoes at Villejuif shows the atmosphere surrounding that station to have been acoustically opaque.

Have we any clue to the cause of the opacity? I think we have. Villejuif is close to Paris, and over it, with the observed light wind, was slowly wafted the air from the city. Thousands of chimneys to windward of Villejuif were discharging their heated currents; so that an atmosphere non-homogeneous in a high degree must have surrounded that station. At no great height in the atmosphere the equilibrium of temperature would be established. The non-homogeneous air surrounding Villejuif is experimentally typified by our screen with the source of sound close behind it, the upper edge of the screen representing the place where equilibrium of temperature was established in the atmosphere above the station. In virtue of its proximity to the screen, the echoes from our sounding-reed would, in the case here supposed, so blend with the direct sound as to be practically indistinguishable from it, as the echoes at Villejuif followed the direct sound so hotly, and vanished so rapidly, that they escaped observation. And as our sensitive flame, at a distance, failed to be affected by the sounding body placed close behind the card-board screen, so, I take it, did the observers at Montlhéry fail to hear the sounds of the Villejuif gun. This is the explanation of Arago's difficulty which I have the honour to submit to the Royal Society.

Something further may be done towards the experimental elucidation of this subject. The facility with which sounds pass through textile fabrics has been already illustrated[†], a layer of cambric or calico, or even of thick flannel or baize, being found competent to intercept but a fraction of the sound from a vibrating reed. Such a layer of calico may be taken to represent a layer of air differentiated from its neighbours by temperature or moisture; while a succession of such sheets of calico may be taken to represent successive layers of non-homogeneous air.

* Phil. Trans. 1874, pt. i. p. 202.
† Phil. Trans. 1874, pt. i. p. 208.

Two tin tubes (M N and O P, fig. 3) with open ends are placed so as to form an acute angle with each other.   At the end of one

Fig. 3.
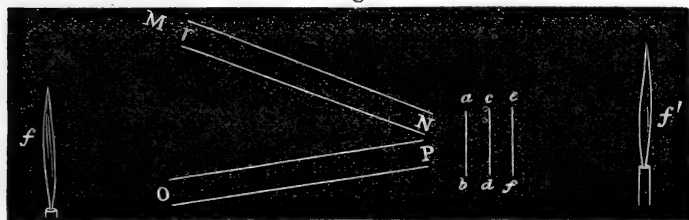
is the vibrating reed *r*; opposite the end of the other, and in the prolongation of P O, is the sensitive flame *f*, a second sensitive flame (*f′*) being placed in the continuation of the axis of M N. On sounding the reed, the direct sound through M N agitates the flame *f′*.   Introducing the square of calico *a b* at the proper angle, a slight decrease of the action on *f′* is noticed, and the feeble echo from *a b* produces a barely perceptible agitation of the flame *f*. Adding another square, *c d*, the sound transmitted by *a b* impinges on *c d*; it is partially echoed, returns through *a b*, passes along P O, and still further agitates the flame *f*.   Adding a third square, *ef*, the reflected sound is still further augmented, every accession to the echo being accompanied by a corresponding withdrawal of the vibrations from *f′* and a consequent stilling of that flame.

With thinner calico or cambric it would require a greater number of layers to intercept the entire sound; hence with such cambric we should have echoes returned from a greater distance, and therefore of greater duration.   Eight layers of the calico employed in these experiments, stretched on a wire frame and placed close together as a kind of pad, may be taken to represent a very dense acoustic cloud.   Such a pad, placed at the proper angle beyond N, cuts off the sound, which in its absence reaches *f′*, almost as effectually as an impervious solid plate[*]: the flame *f′* is thereby stilled, while *f* is far more powerfully agitated than by the reflection from a single layer.   With the source of sound close at hand, the echoes from such a pad would be of insensible duration.   Thus close at hand do I suppose the acoustic clouds surrounding Villejuif to have been, a similar shortness of echo being the consequence.

A further step is here taken in the illustration of the analogy between light and sound.   Our pad acts chiefly by internal reflection.   The sound from the reed is a composite one, made up of partial sounds differing in pitch.   If these sounds be ejected from the pad in their pristine proportions, the pad is acoustically

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[*] January 13th.—Since this was written I have sent the sound through fifteen layers of calico, and echoed it back through the same layers, in strength sufficient to agitate the flame.   Thirty layers were here crossed by the sound.

white; if they return with their proportions altered, the pad is acoustically *coloured*.

In these experiments my assistant, Mr. Cottrell, has rendered me material assistance.

GEOLOGICAL SOCIETY.

[Continued from vol. xlix. p. 481.]

June 24, 1874.—John Evans, Esq., F.R.S., President,
In the Chair.

The following communications were read :—

8. " Description of species of *Chætetes* from the Lower Silurian Rocks of North America." By Prof. H. Alleyne Nicholson, M.D., D.Sc., F.R.S.E., F.G.S.

In this paper the author accepted the union of *Chætetes* and *Stenopora* made by Milne-Edwards and Haime, and stated that *Monticulipora*, D'Orb., and *Nebulipora*, M°Coy, also seemed to him to belong to the same generic group, for which he proposed to employ the name *Chætetes*. The genus thus defined he proposed to divide into 4 groups, under which he described the following North-American species :—

Group I. Ramose species.

C. Dalei, rugosus, pulchellus, and *Fletcheri* (M.-Edw. & Haime); *C. gracilis* (James); and *C. approximatus, attritus, delicatulus, nodulosus, Jamesi,* and *rhombicus,* sp. nov.

Group II. Frondescent and Palmate species.

C. mammulatus (M.-Edw. & Haime), *frondosus* (D'Orb.?), and *clathratulus* (James).

Group III. Massive and Discoid species.

C. petropolitanus (Pand.) and *discoideus* (James); and

Group IV. Incrusting species.

C. papillatus (M°Coy), and *corticans* and *Ortoni,* sp. nov.

Most of the species are from the Cincinnati group near Cincinnati.

9. " On the composition and structure of the Bony Palate of *Ctenodus.*" By L. C. Miall, Esq. Communicated by Prof. P. Martin Duncan, F.R.S., F.G.S.

The specimen noticed by the author was obtained from the Low-Main Coal-seam of Newsham, Northumberland. The component bones are a parasphenoid and a pair of pterygo-palatals. The left dental plate agrees with the type of *Ctenodus cristatus* (Agassiz) in the Leeds Museum. The author describes the bones and teeth in detail. He regards *Ctenodus* as closely related to *Ceratodus* in the structure of the palate, and as differing but little from the Devonian *Dipterus*.

10. " Notes on a Railway Section of the Lower Lias and Rhætics between Stratford-on-Avon and Fenny Compton, and on the occurrence of the Rhætics near Kineton and the Insect-beds near Knowle in Warwickshire, and on the recent discovery of the Rhætics near Leicester." By the Rev. P. B. Brodie, M.A., F.G.S.

This paper consists of a detailed description of the strata exposed at a little distance from the railway-station at Stratford-on-Avon and at Kineton, and of the evidence of the existence of the Insect-beds near Knowle in Warwickshire. The author also records the discovery of the Rhætics near Leicester by Mr. Harrison. The following is the section at the brick-pit at the base of the Spinney Hills, in ascending order—twenty feet of red and blue marls, twenty feet of hard fissured sandy marl containing fish-scales, and fifteen feet of black and light-coloured shales full of fossils. The beds appear to dip to the S.E.

11. " The Resemblances of Ichthyosaurian Bones to the bones of other Animals." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

Hitherto in comparative anatomy the term affinity has been vaguely used in treating of the extinct groups. In this paper the author endeavoured to give precision to the term Ichthyosaurian by analyzing the characters of the Ichthyosaurian skeleton into the resemblances which it presents to the skeletons of other vertebrates. Ichthyosaurian characters are subdivided into Mammalian, Avian, Crocodilian, Chelonian, Lacertilian, Chamæleonian, Rhynchocephalian, Ophidian, Urodelan, Piscine, Plesiosaurian, Dinosaurian, Dicynodont, and Labyrinthodont. By thus classifying the characters it is anticipated that the affinities of the Ichthyosaurian type may be rendered evident.

12. " The Resemblances of Plesiosaurian Bones to the Bones of other Animals." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

This paper is an attempt to make a similar analysis of the Plesiosaurian skeleton.

13. " On the Tibia of *Megalornis*, a large Struthious Bird from the London Clay." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

The author described the distal portion of a right tibia of a large struthious bird from the London Clay of Eastchurch in Sheppey. The only living types approximating to it are the Apteryx, which similarly has the shaft at the back of the distal articulation, and the Emu, which similarly has the shaft compressed from back to front. The author considered that the skull named by Prof. Owen *Dasornis* might, if it belonged to a bird, be referred to *Megalornis*; but he detailed considerations which led him to suggest that *Dasornis* may possibly be a fish.

14. " On Cervical and Dorsal Vertebræ of *Crocodilus cantabrigiensis* (Seeley), from the Cambridge Upper Greensand." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

The author described in detail a cervical and a dorsal vertebra of a procœlous Crocodile from the Cambridge Upper Greensand, which

in the main presented the character of a young existing Crocodile coupled with distinctive features. The centrum of the cervical vertebra is oblique, and the neural canal of the dorsal vertebra is small as in *Hyposaurus*. The species is of small size.

15. " On the base of a large Lacertian Skull from the Potton Sands." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

This specimen was interpreted by the author as the anchylosed basioccipital and basisphenoid of a Dinosaur. It showed a resemblance to Crocodiles in its posterior aspect, but in all other respects was Lacertian ; it makes a close approximation to *Hatteria*, and in no respect shows affinity to birds. The pterygoid processes look downward, and the basioccipital condyle looks downward and backward. The base of the brain-cavity is long and narrow, while its anterior border corresponds to an imperfectly ossified fibrocartilage seen in the same position in *Hatteria*. The author did not regard the specimen as giving support to Prof. Huxley's hypothesis of the Avian affinities of Dinosaurs.

16. " A Section through the Devonian strata of West Somerset." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

In 1867 the author had visited the Devonian country in search of the fault which Mr. Jukes supposed to traverse Devon and West Somerset. He satisfied himself by sectional evidence that no fault existed, and in a section from Hurlstone Point to Brushford has noted down the mineral character of the successive groups of strata forming the country, and the only folds of the strata seen by him on that line of section. He thought that Mr. Etheridge's detailed grouping of the rocks was better suited to the N.W. part of the country than to West Somerset, and that for that region the divisions of strata used by Mr. Jukes were convenient.

17. " On the Pectoral Arch and Fore Limb of *Ophthalmosaurus*." By Harry Govier Seeley, Esq., F.L.S., F.G.S.

After some remarks on the structure of the pectoral arch in *Ichthyosaurus*, the author described parts of a skeleton discovered by Mr. Leeds in the Oxford Clay, on which he founded the genus *Ophthalmosaurus*.

The pectoral arch comprised the usual bones ; but their relation to each other was unusual. The clavicles form a strong arch ; the lateral clavicles join behind by interlacing sutural union, and not by overlap ; while in front the episternum (or interclavicle) is wedged in between them so as to divide them. The coracoids are expanded and grasped by the clavicles in a depression in their margins ; the scapula is large.

The humerus shows the usual Ichthyosaurian characters, and has three facets at the distal end. To these correspond three bones in the forearm, the olecranon-ossification being two thirds as large as the radius. The single row of carpal bones includes four elements.

XIX. *Intelligence and Miscellaneous Articles.*

ON THE TEMPERATURE OF THE SUN.   BY J.-B. SORET[*].

Geneva, 14th Oct. 1874.

I HAVE read with very great interest the Notes, on the temperature of the sun, which you have communicated to the Academy in the name of M. Violle, a distinguished philosopher, who, besides his original works, is entitled to the gratitude of all physicists for the important part he has taken in the publication of Verdet's *Œuvres.* You, perhaps, remember that I have for several years been occupied in researches which, as regards the means of observation, are very similar to those of M. Violle. I have already made known some of the results[†]; and I hope to be able shortly to make these investigations the subject of a more complete publication, which I have been obliged to defer because a correction rather difficult to obtain remained to be determined.

These measurements of the calorific intensity of solar radiation are, I believe, very interesting in divers respects; but I doubt whether, in the present state of science, they can lead to the determination of the temperature of the sun.

The principle of the apparatus, or actinometer, successively employed for these observations, first by Pouillet (who soon adopted a different method), then by Mr. Waterston, Father Secchi, Mr. Ericson, M. Violle, and myself, consists in placing a thermometer with a blackened bulb in an enclosure of which the temperature θ is known. An aperture admits into the enclosed space a pencil of solar rays, which falls upon the bulb of the thermometer, which takes a temperature t.

In order to be able to deduce from this the temperature of the sun, it would be necessary first of all to know the law of the radiation of heat at very elevated temperatures. Sometimes Newton's law has been assumed, sometimes that of Dulong and Petit. Now neither the one nor the other is accurate for high temperatures: this appears to me to follow very distinctly from a series of experiments which I made known two years since[‡]. Permit me to recapitulate the results, referring for the details to the Notes which I have published.

With the actinometer which I employed, the solar radiation at Geneva produces an excess of temperature $t - \theta$ which exceeds $14°\cdot5$. If, instead of exposing the actinometer to the sun, I make use of a disk of zirconium or magnesium heated by the oxyhydric lamp (illuminating-gas and oxygen), placing it at a distance such that its apparent diameter referred to the bulb of the thermometer is the

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[*] Extract from a letter to M. H. Sainte-Claire Deville, published in the *Annales Scientifiques de l'Ecole Normale,* 1874, No. 12.

[†] See *Comptes Rendus de l'Acad. des Sciences,* 1867, vol. lxv. p. 526, and 1868, vol. lxvi. p. 810; *Comptes Rendus de l'Association française pour l'avancement des Sciences,* première session, 1872, p. 282.

[‡] *Archives des Sciences Physiques et Naturelles,* 1872, vol. xliv. p. 220, and vol. xlv. p. 252.

same as that of the sun, I obtain an excess of temperature $t-\theta$ of $0°·5$ only. The temperature of the disk is at least equal to, and probably exceeds, that at which platinum fuses; it may therefore be estimated at 1900 or 2000 degrees.

Father Secchi, a few months ago, communicated to the Academy a similar experiment, made with the electric light, the temperature of which he estimates at 3000°; and he obtained an intensity of radiation from 34 to 36 times less than that of the sun. I am quite disposed to admit the accuracy of this experiment; but one might perhaps object that there is sufficient uncertainty about the temperature of the electric lamp, and that it is not quite certain that the whole extent of the carbon visible to the thermometer is equally heated. I therefore dwell on my own experiment only, against which those objections cannot be urged with the same force.

If Newton's law were strictly accurate, to calculate the temperature T of the disk heated at the oxyhydric lamp, we should have the formula

$$t-\theta=\alpha\mathrm{T},$$

α being equal to $\dfrac{1}{183960}$. Now

$$t-\theta=0°·5,$$

whence

$$\mathrm{T}=91980°,$$

an absolutely inadmissible result; for it is certain that the temperature of the disk is near 2000°, and at any rate does not exceed 2800°. Newton's law, then, is not verified.

The Rev. Father Secchi, in discussing either some of my experiments or his own experiment, infers for the sun a temperature of more than 100,000 degrees. His reasoning may be summed up by saying that, since the intensity of the solar radiation is 44 times that of the electric light, the temperature of the sun must also be 44 times that of the lamp. This conclusion does not appear admissible; for it is equivalent to supposing that Newton's law is accurate from 2000° or 3000° upwards; while experiment manifestly proves that it is inaccurate between zero and 2000°.

Let us pass to the law of Dulong and Petit. From this law M. Vicaire has deduced, for the case we are considering, the formula

$$a^t - a^\theta = \alpha a^\mathrm{T},$$

in which $\alpha = \dfrac{1}{183960}$ as before, and $a=1·0077$. From this we get

$$\mathrm{T}=\frac{\log(a^t-a^\theta)+\log\dfrac{1}{\alpha}}{\log a}.$$

Now, if we apply this formula to my experiment, in which $t=15°·45$ and $\theta=14°·95$, we find $\mathrm{T}=870$, a figure evidently too low, as Father Secchi has already remarked. And if the converse calculation be made by seeking the value of t for a temperature

T=2000°, it is found that the excess $t-\theta$, instead of being $\frac{1}{2}$° conformably to observation, ought to be several hundreds of degrees. Therefore the law of Dulong and Petit, so accurate for temperatures from zero up to 300°, ceases to be so when these limits are passed.

If, by reasoning analogous to that which we have now criticised, we kept the formula

$$a^t - a^\theta = \alpha a^{\mathrm{T}},$$

merely modifying the value of the coefficient a which would be deduced from my experiment, 1·0028 would be found for this value instead of 1·0077. Next, applying the formula to the excess $t-\theta$ which I obtained at the summit of Mont Blanc, we should arrive at the number of 3330° for the temperature of the sun. But the reasoning is not legitimate; the law of Dulong and Petit being inaccurate from 300° to 2000°, it cannot be admitted to be applicable above 2000°. And since the coefficient a (which, according to the law, should be constant) diminishes from 1·0077 to 1·0028 when we pass from 300° to 2000°, it is probable that it would take a still lower value at temperatures exceeding 2000°; and this would conduct us to a higher number than 3330° for the temperature of the sun.

But that is only a probability, not a certainty; and, in brief, I confine myself to saying I do not think we can actually in this way succeed in approximately measuring the temperature of the sun. My impression is, that it is considerably above the highest temperatures we can attain by means of combustion, which are estimated at about 3000°; but does it exceed them by some hundreds, or by some thousands of degrees? That is a question to which I should not like to venture a reply.

In regard to this, permit me to speak again of some trials, made partly in your laboratory at the École Normale, which, imperfect as they are, show once more the great comparative intensity of the solar radiation. If a source of light (a gas-jet, for instance) be viewed through one or several plates of blue cobalt glass, and if the thickness of glass is suitable, the flame appears of a purple tint, from the cobalt permitting the extreme red rays, as well as the blue and violet rays, to pass through, and intercepting the radiations of medium refrangibility. If through the same thickness of glass we observe a luminous source at a higher temperature, and consequently richer in highly refrangible rays, it no longer appears purple, but blue; the thickness of cobalt glass must be increased in order again to obtain the purple tint: in fact, this does not much alter the proportion of red rays transmitted, while the blue rays are sensibly weakened. There is therefore a relation between the thickness of cobalt glass which produces the purple tint and the temperature of the source, at least if we have to do with white light emanating from an incandescent solid or liquid body. With some improvements, one might base on this principle the construction of a sort of pyrometer which would perhaps be useful in certain cases.

The following are some results which I have obtained by operating with plates cut out of one and the same cobalt glass. At the temperature of fusion of platinum, two of these plates superposed were sufficient to give sensibly the purple tint : this I have been able to observe leisurely at the Conservatoire des Arts et Métiers at the time of the fusion of the platinum for making the international mètres, at which operation I had the good fortune to be present. Shortly after, thanks to your kindness, I had the opportunity of attending in your laboratory at the experiment of the fusion of iridium : at the instant of the maximum of temperature, this source of light, viewed through the same two plates, appeared perfectly blue ; but with three plates the tint was purple, of a shade exactly like that given by a gas-flame seen through two plates. Now, if the sun is observed high above the horizon in a clear sky, the purple tint is not obtained with three, or four, or even with six plates superposed. A great thickness of cobalt glass is requisite in order to get this shade on the edge of the sun's disk ; and part of the effect is doubtless to be attributed to defects of homogeneity in the glass which I employed. The light of the moon gives the same result, showing that the intensity has no influence.—*Bibliothèque Universelle, Archives des Sciences,* Feb. 1875.

ON FUSED BORACIC ACID AND ITS TEMPERING.
BY V. DE LUYNES.

Fused boracic acid, which closely resembles glass in external characters, presents the following remarkable properties.

In the viscous state it can be drawn out in threads which rapidly solidify ; and in this respect its ductility resembles more that of silica than that of glass properly so called.

Its hardness, between 4 and 5, places it between fluorine and apatite ; it scratches glass ; it is found to be with difficulty cut by means of sand, grit, or emery either dry or with oil : to grind it the solvent action of water must be interposed ; and in this case the cutting takes 7 or 8 times as long as that of common glass in the same circumstances. This resistance to grinding, which does not correspond to its hardness, is doubtless due (as M. Damour has ascertained in the case of other minerals) to a peculiarity of structure.

Fused boracic acid in mass is slowly hydrated in contact with water ; but when pulverized the action is rapid, as Ebelmen has shown. When powdered boracic acid is moistened with water, it increases in volume, and the temperature of the mixture rises to nearly 100°.

Boracic acid is remarkable especially for the energy and persistence of its tempering. By running it upon a cold metallic surface vitreous plates are obtained, of which the lower surface, cooled by the metal, is more strongly tempered, and consequently more dilated than the upper surface. Hence results a flexure which may be sufficiently great to determine the rupture of the plate and its

projection in splinters. Boracic acid poured upon water is suddenly pulverized; but with oil little masses with short stems are obtained which explode under the same conditions as Prince Rupert's drops.

A chilled plate of boracic acid with parallel surfaces acts on polarized light like chilled glass; but while the latter loses this property by annealing, boracic acid retains it with singular tenacity. Some fragments of chilled boracic acid were placed in the annealing-furnaces of M. Feil, kept at a red heat for fifteen hours, and submitted to a slow cooling of several days. They acted on polarized light as energetically as before, although some lumps of glass of 60 kilogrammes placed beside them were completely annealed.

On placing some rather large pastils of chilled boracic acid in water at the temperature of from 15° to 20°, hydration is observed to take place in layers, producing a veritable exfoliation. The inner layers, in undergoing hydration, increase more in volume than the outer ones; hence results a lifting of the exfoliated portions, which takes place nearly symmetrically with respect to the middle layer towards each surface; and the plate of boracic acid, after hydration, has the appearance of two caps tangent by their convex surfaces.

This deformation is constant, and does not depend on the form of the fragment of boracic acid operated on. It proves that the two surfaces are chilled in opposite directions; it shows, next (and this is the most interesting fact), that the portions already dilated by chilling do not undergo through combination with water the same augmentation of volume as the parts less chilled or which are not chilled at all. In a word, hydration produces in boracic acid augmentation of volume; nevertheless, if tempering has already given rise to an increase of volume, hydration takes place, but there is not the same change of volume. These facts appear to be intimately connected with those described by M. Berthelot under the name of *kénomérie.* They confirm, as regards boracic acid, the structure which I have attributed to chilled glass.—*Comptes Rendus de l'Acad. des Sciences,* vol. lxxxi. pp. 80, 81.

THE DISCOVERY OF A METHOD OF OBTAINING THERMOGRAPHS OF THE ISOTHERMAL LINES OF THE SOLAR DISK. BY ALFRED M. MAYER.

On June the 5th, 1875, I devised a method for obtaining the isothermals on the solar disk. As this process may create an entirely new branch of solar physics, I deem it proper that I should give a short account of it in order to establish my claim as its discoverer.

In the American Journal, July 1872, I first showed how one can, with great precision, trace the progress and determine the boundary of a wave of conducted heat in crystals, by coating sections of these bodies with Meusel's double iodide of copper and mercury and observing the blackening of the iodide where the

wave of conducted heat reaches 70° C. If we cause the image of the sun to fall upon the smoked surface of thin paper while the other side of the paper is coated with a film of the iodide, we may work on the solar disk as we formerly did on the crystal sections.

The method of proceeding is as follows :—Beginning with an aperture of object-glass which does not give sufficient heat in any part of the solar image to blacken the iodide, I gradually increase the aperture until I have obtained that area of blackened iodide which is the smallest that can be produced with a well-defined contour. This surface of blackened iodide I call the *area of maximum temperature*. On exposing more aperture of object-glass, the surface of blackened iodide extends and a new area is formed bounded by a well-defined isothermal line. On again increasing the aperture another increase of blackened surface is produced with another isothermal contour ; and on continuing this process I have obtained maps of the isothermals of the solar image. By exposing for about 20 minutes the surface of iodide to the action of the heat inclosed in an isothermal, I have obtained thermographs of the above areas, which are sufficiently permanent to allow one to trace accurately their isothermal contours. There are other substances, however, which are more suitable than the iodide for the production of permanent thermographs.

The contours of the successively blackened areas on the iodide are *isothermals*, whose successive thermometric values are inversely as the successively increasing areas of aperture of object-glass which respectively produced them.

As far as the few observations have any weight, the following appear to be the discoveries already made by this new method :— (1) There exists on the solar image an area of sensibly uniform temperature and of maximum intensity. (2) This area of maximum temperature is of variable size. (3) This area of maximum temperature has a motion on the solar image. (4) The area of maximum temperature is surrounded by well-defined isothermals marking successive gradations of temperature. (5) The general motions of translation and of rotation of these isothermals appear to follow the motions of the area of maximum temperature which they inclose ; but both central area and isothermals have independent motions of their own.

On projecting the enlarged image of a sun-spot on the blackened surface and then bringing a hot-water box, coated with lamp-black, near the other side of the paper, one may *develop* the image of the spot in *red* on a dark ground. A similar method probably may serve to develop the athermic lines in the ultra-red region of the solar and other spectra.—Silliman's *American Journal*, July 1875.

XX. *Note on* Kohlrausch's *Determination of the Absolute Value of the Siemens Mercury Unit of Electrical Resistance.* By HENRY A. ROWLAND, *of the Johns Hopkins University, Baltimore, Md.*[*]

IN looking over Kohlrausch's paper[†] upon the determination of a resistance in absolute measure, with a view to undertaking something of the kind myself, and also, if possible, to discover the reason of the difference from the results of the Committee of the British Association, I think I have come across an error of sufficient magnitude and in the proper direction to account for the 2-per-cent. difference. Kohlrausch's experiments were made with such great care and by so experienced a person that it is only after due thought and careful consideration that I take it upon me to offer a few critical remarks.

We observe, then, first of all, that the principal peculiarity of his method consists in doing away with all measurements of the coils of the galvanometer, and in its place making accurate determinations of the logarithmic decrement both with the circuit closed and open, together with various absolute determinations rendered necessary by this change. In this way the logarithmic pecrement is raised from being a small correction to a most imdortant factor in the equation. Hence it is that we should carefully scrutinize the theory and see whether it be correct enough for this purpose; for only an approximation is needed for the first method.

The resistances to a bar magnet swinging within a coil may be

divided into two principal parts—first, that due to the resistance of air and viscosity of suspending fibre, and, second, that due to the induced current in the coils. The first resistance is usually taken as proportional to the velocity, and thus assumes the viscosity of the air to be the most important element. This is probably true in most cases where the motion is slow. This factor is quite small compared with the second when the magnet is large and heavy and the coils wound close to it, as in Kohlrausch's instrument. Kohlrausch's principal error lies in the omission of the coefficient of self-induction from his equations.

For the sake of clearness, and because the subject is quite often misapprehended, I shall commence at the beginning and deduce nearly all equations.

Let us proceed at first in the method of Helmholtz, using the notation of Maxwell's 'Electricity.'

Let a current of strength I be passing in a circuit whose resistance is R, and coefficient of self-induction L. Also let a magnet be near the circuit whose potential energy with respect to the circuit is IV. Let A be the electromotive force of the battery in the circuit.

The work done by the battery in the time dt is equal to the sum of the work done in heating the wire, in moving the magnet, and *in increasing the mutual potential of the circuit on itself*[*]. Hence we have

$$A I dt = I^2 R dt + I \frac{dV}{dt} dt + \frac{1}{2} L \frac{d(I^2)}{dt} dt;$$

and if A is equal to zero, we find

$$I = -\frac{1}{R}\left(\frac{dV}{dt} + L\frac{dI}{dt}\right).$$

If we apply this to the case of a magnet swinging within a coil, the angle of the magnet from a fixed position being x, we have since $\frac{dV}{dx}$ is the moment of the force acting on the magnet with unit current and may be denoted by q,

$$I = -\frac{1}{R}\left(q\frac{dx}{dt} + L\frac{dI}{dt}\right), \quad . \quad . \quad . \quad . \quad (1)$$

where my R is Kohlrausch's w.

This expression differs from that used by Kohlrausch in the addition of the last term, which is the correction due to self-induction. The last term vanishes whenever the magnet moves with such velocity as to keep the induced current constant; but in the swinging of a galvanometer-needle it has a value.

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[*] See remarks in Maxwell's 'Electricity,' art. 544, near bottom of page.

To form the equation of motion of the needle, we can proceed the rest of the way as Maxwell has done (Electricity, art. 762). Assuming that all frictional resistances to the needle are proportional to the velocity of the needles, we have

$$\mathrm{B}\,\frac{d^2x}{dt^2} + \mathrm{C}\,\frac{dx}{dt} + \mathrm{D}x = q\mathrm{I}, \quad . \quad . \quad . \quad . \quad (2)$$

where B, C, and D are constants.

Eliminating I between this equation and (1), we find

$$\left(\mathrm{R} + \mathrm{L}\,\frac{d}{dt}\right)\left(\mathrm{B}\,\frac{d^2x}{dt^2} + \mathrm{C}\,\frac{dx}{dt} + \mathrm{D}x\right) + q^2\frac{dx}{dt} = 0. \quad . \quad . \quad (3)$$

At first sight this equation will appear to be the same as that of Maxwell; but on further examination we see that it is more general in the value of q.

Equation (3) is the correct equation to use in this case, and reduces to that of Kohlrausch when $\mathrm{L} = 0$.

To see how this error will affect Kohlrausch's results, we must remember that he uses this equation to find the constant of his galvanometer, on which his whole experiment depends; and the **error** is so interwoven with all his results that an entire recomputation is necessary, provided the data for calculating the coefficient of self-induction of the galvanometer coils and earth inductor can be obtained.

The equation

$$\frac{t^2}{\pi^2 + \lambda^2} = \frac{t_0^2}{\pi^2 + \lambda_0^2}$$

does not hold when self-induction is considered; and so his fundamental equation (1) is not correct, containing a twofold error.

The linear differential equation (3) is easily solved; but as the results are complicated, it is hardly worth while at present, until a recalculation can be made. I prefer to solve it on the supposition that L is small, and thus merely obtain a correction to Kohlrausch's equation connecting t and t_0, after which equation (15) or (17) (Maxwell's 'Electricity,' art. 762) can be used when made more general by substituting q for Gm.

As far as I have had time to go at present, the correction seems to be in the direction of making Kohlrausch's determination more nearly coincide with that of the Committee on Electrical Standards of the British Association. Other engagements occupy my attention at present; but I hope to see these corrections made to an otherwise excellent determination of this most important unit.

London, August 4, 1875.

XXI. *On Temperament, or the Division of the Octave.*—No. II.
By R. H. M. Bosanquet, *Esq.**

[Continued from vol. xlviii. p. 511.]

BEFORE continuing the discussion of the subject in the form proposed at the end of my last communication, I have to say a few words on the general purpose of the investigations contained in these papers.

Some time ago, in tuning an organ, my attention was directed, for the first time in a practical manner, to the effect of temperament. Since that time I have perceived the effect constantly in all musical instruments, in some more, in some less. I do not say that the effect is always tormenting or intolerable; but it is there. This was the inducement to study the subject in the first instance.

The following passage from Stainer's ' Harmony founded on the Tempered Scale,' pointed out a defect in our knowledge with a directness rarely found in the writings of any one not specially devoted to science.

" When musical mathematicians shall have agreed amongst themselves upon the exact number of divisions necessary in the octave, when mechanists shall have constructed instruments upon which the new scale can be played, when practical musicians shall have framed a new notation," &c.

Without entering into detailed criticism, I may observe that this passage first pointed out to me the division of the octave as a subject of study : it was clear that musicians in general knew nothing about it, and scientific men but little more, and that any knowledge that did exist was perfectly useless regarded from any point of view whatever.

Two principles are regarded as axioms, or perhaps postulates, drawn from practical music, especially that of Bach :—

    (1) Similarity of all keys.
    (2) Unlimited facility of modulation.

The first requirement is met by the property which I have expressed by the word regular—*i. e.* that the notes of a system admit of arrangement in a series of fifths, all of which have the same value. The second requirement can only be completely met by the property to which the name cyclical has been commonly applied.

All will acknowledge that the notes of the ordinary equal temperament are first approximations to the notes intended to be discussed; and it is in strict analogy to the practice of other

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* Communicated by the Author, being the substance of a second paper read before the Musical Association, May 3, 1875.

branches of experimental science, the history of which invariably indicates a procedure by successive approximations, that I reduce the discussion to that of corrections to, or departures from, equal-temperament positions.   I am obliged to mention this, as there is a tendency on the part of some, especially pure mathematicians, to object to the treatment by approximations.   But we must remember that there is no instance of any theory being reduced to observation or experiment except by means of approximation, or with a limited accuracy of some kind; and astronomy itself, the greatest example of modern science, is the greatest example of the procedure of approximations.

We can easily imagine the octave divided into any number of parts.   And we can easily imagine that *a* of these parts will be the best approximation to the fifth, *b* to the third, and so on; and if we tabulated all the values for all possible modes of dividing the octave, we should no doubt be able to find out all the properties of every division.   But this would be a very clumsy proceeding; and accordingly I have preferred to start from a quasi-analytical point of view, and to define a number having certain relations with any system, which I call its order.   This admits readily of doing two things :—first, determining the best approximation to the fifth; and secondly, arranging the whole system into scales corresponding to that approximation.   Thirds and other intervals are made to depend upon the fifths.   Now the definition of the order of a system is really simple.   Since the equal-temperament fifth is 7 semitones and the octave is 12, it is clear that 12 equal-temperament fifths = 7 octaves.   Hence the deviation of 12 fifths of any system from 7 octaves is an important property of the system; and if we take some approximate fifth of any system and multiply its number of units by 12, the number of units by which the result exceeds or falls short of 7 octaves is what I call the order of the system for that approximate fifth.   With ordinary systems there is only one approximation that merits discussion at all, and consequently only one order practically for each system.   The importance of the order is not only that systems are arranged into scales in different manners, according to their orders, but also, since the departure of the fifth of the system *n* of order *r* is $\frac{r}{n}$, it is only necessary to compare this fraction with the departure of the perfect fifth (·01955), or of any other fifth, to ascertain with facility all that is to be known about it.

Having said so much as to the general method, I pass on to notice a few points in the history of the subject.   For general references, see Mr. A. J. Ellis, F.R.S., "On the Temperament of Instruments with Fixed Tones" (Proc. Roy. Soc. 1864).

The history of the scale divides itself into three principal periods, corresponding to (1) the ancient systems of perfect concords, (2) the mean-tone and allied temperaments, and (3) the equal temperament.   The exact mode of origin of the first temperaments is no doubt unknown.   According to Smith ('Harmonics,' 1759), Salinas found a temperament in use in Italy in an empirical manner, its theory not being understood; and on investigating the question, he arrived at certain forms, that which he preferred being such that all the fifths were made $\frac{1}{4}$ of a comma flat; this is the mean-tone system.   Zarlino is said to have first published it; so that the date of the origin of this system is ascertained with some certainty as being in the latter part of the sixteenth century.   Its practical employment does not appear to have spread very rapidly.   Mersenne (*Harmonicorum Liber*, 1636), who quotes Salinas, and certainly knew of the system, confines himself almost entirely to perfect-concord system.   A century later the mean-tone system was almost universal (though Bach had by that time given it its death-blow as applied to 12-keyed instruments); and in Smith's 'Harmonics' it, and modifications of it, are treated as being alone of any value. The equal temperament, or the division of the octave into 12 equal intervals, has now almost universally superseded it.

Mersenne himself belongs to the first period.   His systems are all based on perfect concords; and he meets the difficulty of the great number of notes required for such systems by means of key-boards of greater or less complexity.   These are in principle of the same nature with the key-boards of General T. Perronet Thompson; and indeed Thompson says himself that he learnt much from Mersenne.

The system of 31 equal divisions in the octave is known by Huyghens's name.   This system was known to Mersenne and Salinas; but they could not make any thing of it.   Huyghens points out that they were not acquainted with the methods required for dealing with it, and constructs it himself with the aid of logarithms (*Cyclus Harmonicus—Opera Varia*, vol. i.).   All parts of this system are, within an error small with respect to the error of practical tuning, the same as the mean-tone system. The original date of the *Cyclus Harmonicus* was probably before 1700.

Smith's 'Harmonics,' 1759 (2nd edit.) has always been regarded as a most important work.   A curious remark in the preface deserves attention.   It amounts to a numerical coincidence between the ratios interval of octave : interval of major third, and circumference of circle : diameter.   Of course the coincidence is purely accidental, both ratios being a little greater than 3; but that a practical man should, as Smith relates,

have adopted this as a rule for tuning seems curious. Smith discusses only systems with one class of whole tones (*i. e.* those called by Mr. Ellis "commatic"); and practically he confines himself to negative systems (*i. e.* systems with fifths flatter than equal-temperament fifths). He regards perfect-concord systems as quite unmanageable for practical purposes; and, admitting the restriction to 12 sounds per octave, there is no doubt he was right. His fundamental theorem is as follows:—The octave, a constant interval, being made up of five tones and two semitones, if the tone and semitone be changed in any manner, the change of the tone is to that of the semitone as 2 : 5. Now in all such systems the tones are "two-fifths tones," *i. e.* are got by tuning two fifths up, and the semitones are five-fifths semitones, *i. e.* are got by tuning five fifths down, whence Smith's result follows independently.

Smith's criterion for systems consists in the consideration of the dissonances of the tempered fifth, sixth, and third. He ascribes to each interval a measure of dissonance, *i. e.* supposes that tempering by the same interval produces different dissonances in different concords. It is for the purposes of this measure that his investigation of the theory of beats is undertaken. The work of Helmholtz has superseded his theory of beats for practical purposes. Of the three temperaments Smith arrives at, the one which he calls equal harmony is that which he prefers. It makes the fifths flatter than in the mean-tone temperament, and the thirds flat; the fifths and sixths beat equally in opposite directions. The mean tone he regards as preferable in the next degree; he calls it the "vulgar temperament." But as both of these are very bad on the 12-keyed board, he proposes a third system in which the thirds and fifths beat equally fast, the fifths flat and the thirds sharp. This system is intermediate between the mean-tone and the equal-temperament; and the interest of it is that it, or something very like it, appears to have been generally adopted in practice. The more recent form of the unequal temperament is substantially this system, though it may be doubtful whether its employment was derived from Smith*. The properties of Smith's equal harmony are closely represented by the cycle of 50 (negative of the second order), as he himself points out; and his third system (that above alluded to) has very nearly the properties of the system of 43 (negative of the first order). Smith describes an arrangement of stops for introducing additional notes into the harpsichord for the purposes of the mean-tone system or of his "equal harmony;" he possessed and used a harpsichord thus constructed. Mr. Ellis has recently employed the same method; but my opinion

* See 'Hopkins on the Organ,' p. 183.

is that any mechanism, whether stops or pedals, for the selection of the notes required is unpractical in relation to the rapid modulations of modern music.

There is an interesting passage in which Smith anticipated to some extent the doctrine of Helmholtz that dissonance consists of beats (p. 227):—"For nothing gives greater offence to the hearer, though ignorant of the cause of it, than those rapid rattling beats of high and loud sounds which make imperfect consonances with one another; and yet a few slow beats, like the slow undulations of a close shake now and then introduced, are far from being disagreeable."

I may here remark that there is a discrepancy between the practice of musicians and that introduced by Helmholtz as to the employment of the term dissonance. The principal authorities in technical music are unanimous in regarding a fourth as a dissonance. I propose to call such combinations as the fourth or harmonic seventh, neither of which gives sensible beats, "*unsatisfied combinations.*" Dissonances may then be divided into beating dissonances and unsatisfied combinations.

Woolhouse's 'Essay on Musical Intervals' (1835). This writer uses equal-temperament semitones for the first time. The principal systems he discusses are those of 50, 31, and 19, all negative.

In a paper by De Morgan " On the Beats of Imperfect Consonances " (Camb. Phil. Trans. vol. x. p. 129), equal-temperament semitones are employed as the measure of interval. The rules there given for the transformation of logarithms of vibration-ratios into equal-temperament semitones, and *vice versâ,* are substantially the same with those I employ. The treatment of the problems of beats and of resultant tones is superseded by Helmholtz's work.

In a paper by Herschel (Quarterly Journal of Science, vol. v. p. 338) various scales are proposed. In one of these, which is preferred, all the fifths except one are perfect, the remaining one erring of course by a Pythagorean comma. His observation is, " The chief blemish is the paucity of perfect thirds of both kinds; but, on the other hand, none of them err in excess or defect beyond a comma." The defective fifth is taken to be $d-\diagdown a$ (Table at top of p. 348, column D, row *sol*). Now an error of a comma in a fifth makes it unfit for use in music; no ear can tolerate a fifth which is a comma out of tune; so that this arrangement would exclude from use the keys of G, D, and A major, to say nothing of minor keys.

I here pass over the instruments of Mr. Poole* and General

T. Perronet Thompson. Their examination would involve much technical detail. They both represent forms of just intonation.

I need not enter into any detail as to Helmholtz's work in this Magazine, more especially as it is now accessible in an English translation. But I must allude to a serious defect in Tyndall's exposition of Helmholtz's theory of beats in the ear, to which my attention was drawn by Professor Mayer's paper in the 'American Journal of Science,' October 1874, though I believe it had been commented on before by Mr. Sedley Taylor.

In 'Tyndall on Sound' (2nd edit. p. 296) we find the following statement as the reason why no beats are audible in the octave c_1-c_2:—

" Here our rates of vibration are 512, 256; difference $=256$. It is plain that in this case we can have no beats, the difference being too high to admit of them."

This is not Helmholtz's position. His theory is that notes an octave apart affect different portions of the nervous mechanism of the ear, and consequently no beats ever take place between these sounds at all when they are received in the ear.

Again, Helmholtz does not say, as Tyndall makes him, that beats blend always into a continuous sound when they attain the limit of 132 per second. He says (3rd edit. p. 270) of the 132 beats per second produced by the interval $b_3 c_4$, " and these are really audible in the same manner as the 33 beats of $b_1 c_2$, although they sound somewhat weaker in the higher position."

An account of some repetitions of the experiments of Professor Mayer contained in the paper above referred to appeared recently in the Philosophical Magazine. They consist of determinations of the limiting rates at which beats cease to be distinguished for all parts of the musical scale.

Mr. Ellis's paper in the ' Proceedings of the Royal Society ' for 1874 deals almost exclusively with just intonation. The remark I would make with reference to this is, that the chief difficulty in applying just or approximately just systems to ordinary music lies in the music itself; this does not conform to certain rules which must be observed in writing for just intonation; and I feel sure that until just intonation is studied, and the fact that it must be specially written for is recognized, no real benefit can be got from its employment.

A question of primary importance, which has perhaps scarcely received sufficient attention, is, Is there any natural standard for excellence in melodic sequences? and if so, what is it?

Mr. Ellis (Proceedings of the Royal Society, 1864) and, I believe, Mr. Sedley Taylor, accept Helmholtz's authority for the position that the sequences of the diatonic scale are the best, and that in proportion as the sequences of any other scale differ

from those of the diatonic scale, they will offend the ear. Helmholtz adduces, in support of this view, an experiment conducted by him and Herr Joachim with the assistance of Helmholtz's harmonium with pure scales. The result appeared to prove conclusively that the eminent violinist employed pure scales. And it is sought to infer from this that the sequences of the diatonic scale commend themselves naturally to the ear in all cases. This is more than can be legitimately deduced from the experiment. There is no doubt that good violinists play substantially perfect concords, in general at least. It is clear that they cannot do so in all cases; for there are, as I have said, many passages in ordinary music which do not admit of being constructed with perfect concords. And the artist who habitually plays perfect concords (when away from the piano), may very well play single notes in the same way. But there is no doubt, in the first place, that all violinists play differently when accompanying a keyed instrument; and in the second place, the experiments of Cornu and Mercadier, of which Mr. Ellis has given an account, showed that the artists, whose performance was there examined, generally played their thirds very sharp when freed from accompaniment, though they played them true when the harmony required it.

But this seems to me eminently a question in which progress may be made. It is commonly treated as if it were impossible to bring any observation of one's own to bear upon the matter; but I have come to the conclusion that a process of education, such as my own ears have gone through, and constantly repeated experiments, such as those I have been in the habit of making with the assistance of persons possessing ears of the most exceptional delicacy, are necessary to enable an opinion to be formed on the matter. I do not think that any single experiments can compare as evidence with such as have been repeated day after day for a long period, until the effects to be observed have become perfectly familiar.

First, as to diatonic scales. The most striking effect of the diatonic scale is, to my ear, the difference between the major and minor tones: this difference is a comma; and I now perceive it with perfect clearness. The impression it makes varies according to whether I have been using the diatonic scale much or not. If I come back to the instrument (the 53 harmonium, which has diatonic scales within a very close approximation) after a considerable absence, and especially if I have been hearing much ordinary music, the impression made by the unequal tones is disagreeable. When I first realized the difference I thought it horrible. Few, except highly gifted musicians, can realize this difference without frequent repetition of the experiments; but

Mr. Hullah, for instance, who caught it at once, thought it very disagreeable. I have come to like it, but only as a consequence of custom, and by attending rather to the smoothness of the chords than to the melody. Mr. Parratt, organist of Magdalen College, Oxford, who has a most acute ear in every respect, and possesses the perception of absolute pitch in a very high degree[*], has frequently listened to these sequences. The result is always the same; he cannot tolerate any of them. They are different in magnitude from the sequences he employs; and, as he describes it, when he hears one of these intervals he also hears clearly in the mind the note that would be given by the interval he is accustomed to, *i. e.* the equal-temperament interval; and the result is to him intolerable. There is no doubt also about the chords; the note most objected to in chords formed from the diatonic scale is the minor third. Many musicians have singled this out instantly as offensive. In fact it is about one sixth of an equal-temperament semitone higher than the equal-temperament note would be. On evidence of this kind I refuse to believe that the diatonic scale is a natural standard of melody.

A strong piece of evidence in the same direction is obtained from the mean-tone system. The semitone of that system is one sixth part greater than an equal-temperament semitone (1·171). The best musicians perceive this at once; indeed I perceive it clearly myself. This effect was, when I first realized it, decidedly unpleasant; and the dislike manifested to this semitone by those I really trust, is such as to convince me that to them it is intolerable. Now the diatonic semitone is 1·117; it is consequently nearer to the mean-tone semitone (1·171) than to the equal temperament (1·0); and if the diatonic semitone were really the standard, the mean tone should be better than the equal-temperament one, which is contrary to the fact.

The mean-tone system was placed on one stop of a small organ with my generalized key-board, which was exhibited to the Musical Association on May 3. Three of Bach's preludes were played on this system as examples; and the above point about the semitone came out strongly in the remarks which were made. The other stop was tuned on the so-called just system (Helmholtz's).

I may observe that these points about melodic sequences cannot be experimented on to any purpose, unless performance of some development is carried out. If I had been content to sound a few chords instead of showing what effect would be produced by music of some development, I should have lost the most conclusive piece of evidence I have so far obtained.

[*] The existence of this power is a great guarantee for trustworthiness in these observations.

The view to which I have been driven is that the appreciation of melodic sequences is purely the result of custom. This completely explains how an eminent violinist may play diatonic scales, while other musicians, educated at a keyed instrument, cannot endure any marked deviation from the equal temperament.

The harmonic seventh is an interesting example of the general point. If we flatten the tempered minor seventh by about one third of a semitone, we get a pure chord free from beats. The ordinary musician cannot at first endure it. The consonance of a full chord of the dominant seventh containing this note is of the most magnificent quality; and if its introduction is properly managed, so that the interval made by the flattened note with the preceding and following notes of the same part is not outrageous, most musicians appreciate its beauty after a time. But it is necessary to be very careful about the context. Though by dint of custom I like the chord much now, yet if I introduce it so as to give rise to any melodic interval widely different from its usual value, the effect is still bad to my ear.

I am clear that a general certainty on this and similar points can be attained only by a wider diffusion of actual knowledge (not reading) on the subject. One might suppose that an educated musician would have some right to speak with authority on such questions as intervals, consonance, dissonance, and the like. It is hard to believe, but it is nevertheless true, that scarcely any musician has ever heard a perfect consonance, or has any idea of the sound of any intervals except those formed by the equal-temperament scale. But until a person has become tolerably familiar with the effects of any system, his opinion as to its merits cannot command any confidence. Smith, in his ' Harmonics,' makes this same observation. These systems can all be thoroughly studied by means of my key-board; whether they ever will be so studied remains to be seen.

Intervals.

Intervals are always expressed in terms of equal-temperament (E.T.) semitones. The following rules are employed for transformation of vibration-ratios into E.T. semitones as far as five places of the latter, and *vice versâ*. The rationale is obvious.

I. To find the value of a given vibration-ratio in E.T. semitones.

From the logarithm of the ratio subtract $\frac{1}{300}$ of itself; call this the first improved value. From the logarithm subtract $\frac{1}{300}$ of the first improved value, and $\frac{1}{10000}$ of the first improved value. Multiply the result by 40. The product is the value in E.T. semitones correct to five places.

The values of the perfect fifth and third correct to ten places are :—

$$\text{Fifth} = 7{\cdot}01955\ 00086,$$
$$\text{Third} = 4{\cdot} - {\cdot}13686\ 28614.$$

II. To find the vibration-ratio of an interval given in E.T. semitones.

To the given number add $\frac{1}{300}$ and $\frac{1}{10,000}$ of itself; divide by 40. The quotient is the logarithm of the required ratio.

The ratio of the E.T. third thus found is $1{\cdot}125995 : 1$.

Intervals of Regular Systems.

These intervals are formed by proceeding through a certain number of fifths, disregarding octaves. Thus we have, representing a fifth by $7 + \delta$,

$$
\begin{aligned}
\text{Departure of 12 fifths} &= 12(7+\delta)-84=12\delta,\\
\text{2-fifths tone} &= 2(7+\delta)-12=1+2\delta,\\
\text{7-fifths semitone} &= 7(7+\delta)-48=1+7\delta,\\
\text{5-fifths semitone} &= -5(7+\delta)+36=1-5\delta,\\
\text{4-fifths third} &= 4(7+\delta)-24=4+4\delta,\\
\text{8-fifths third} &= -8(7+\delta)+60=4-8\delta.
\end{aligned}
$$

Putting for δ its value for any given system $(\delta = \dfrac{r}{n}$ for any cyclical system), we have at once the values of these intervals. Any others can be formed in a similar manner. The 4-fifths third is that of systems which Mr. Ellis calls " commatic ;" I reject it as useless except in negative systems. The 8-fifths third is that of systems which Mr. Ellis calls " skhismatic ;" I employ it in positive systems, according to Helmholtz's proposal.

Positive Systems.

For the details relating to the treatment of these scales, which are somewhat technical, I must refer to the ' Transactions ' of the Musical Association ; I will here only allude to the double second of the key.

In any positive system the second of the key may be derived in two ways :—first, as a fifth to the dominant (by 2 fifths from the key-note); and, secondly, as a major sixth to the subdominant (by 10 fifths down from the key-note). Thus the first second to c is d, the other $\diagdown d$. On account of the importance of this double form of second, I will consider the derivation of these two forms by the ordinary ratios, *i. e.* for just intonation ; the positive systems furnish approximations to the same results.

Two fifths up and an octave down give $\left(\dfrac{3}{2}\right)^2 \times \dfrac{1}{2} = \dfrac{9}{8}$. One

fifth down gives the subdominant, and a sixth up the depressed second, or $\frac{2}{3} \times \frac{5}{3} = \frac{10}{9}$; and the ratio of the two notes in question is $\frac{9}{8} \div \frac{10}{9} = \frac{81}{80}$, an ordinary comma.

The following example illustrates the difficulty thus introduced into ordinary music, in which the depressed second is not recognized.

It is impossible to assign a position for the note marked ∗ which does not involve a fourth or fifth faulty to the extent of a comma; and, according to my experience, this cannot be tolerated. It is necessary therefore to avoid such combinations in dealing with positive systems.

The little circle (o) is used to neutralize the signs of elevation and depression in exactly the same way as (♮) neutralizes sharps or flats in ordinary music.

Harmonic Seventh.

If we compute (rule I.) the interval of the harmonic seventh (7 : 4), we find 10 − ·31174; or it is less than ten semitones by about one third of a semitone. In fact it is well known that if we flatten a minor seventh by some such quantity, we obtain a smooth combination free from beats. We can obtain a pretty close representation of this by taking 14 fifths down in positive systems according to an approximation pointed out by Helmholtz, or by 10 fifths up in negative systems (Mr. Ellis).

Rule. The harmonic seventh on the dominant must not be suspended, so as to form a fourth with the key-note.

In ratios this stands as follows:

Tonic : dominant : : 4 : 3,
Dominant : h. seventh : : 4 : 7;
∴ tonic : h. seventh of dominant : : 16 : 21 : : 48 : 63.

But

Fourth : tonic : : 4 : 3 : : 64 : 48;
∴ fourth : h. seventh of dominant : : 64 : 63;

and the error of the h. seventh of the dominant, if treated as a fourth to the tonic, is more than a comma. This implies a great restriction on the use of the harmonic seventh.

Negative Systems.

The scales of negative systems form themselves very simply according to the notation of ordinary music. It will be sufficient to say here that, except in case of the employment of the approximate harmonic seventh, the distinction between flats and sharps is sufficient to indicate the proper treatment of these systems.

Concords of Regular and Regular Cyclical Systems.

Name or n.	Order.	Departure of fifth.	Departure of third.	Departure of H. seventh.
29	1	$\dfrac{1}{29}$	$-\dfrac{8}{29}$	$-\dfrac{14}{29}$
41	1	$\dfrac{1}{41}$	$-\dfrac{8}{41}$	$-\dfrac{14}{41}$
Perfect fifths.	...	$\dfrac{1}{51\cdot151}$	$-\dfrac{8}{51\cdot151}$	$-\dfrac{14}{51\cdot151}$
53	1	$\dfrac{1}{53}$	$-\dfrac{8}{53}$	$-\dfrac{14}{53}$
Positive perfect thirds.	...	$\dfrac{1}{58\cdot454}$	$-\dfrac{8}{58\cdot454}$	$-\dfrac{14}{58\cdot454}$
118	2	$\dfrac{1}{59}$	$-\dfrac{8}{59}$	$-\dfrac{14}{59}$
65	1	$\dfrac{1}{65}$	$-\dfrac{8}{65}$	$-\dfrac{14}{65}$
43	-1	$-\dfrac{1}{43}$	$-\dfrac{4}{43}$	$-\dfrac{10}{43}$
31	-1	$-\dfrac{1}{31}$	$-\dfrac{4}{31}$	$-\dfrac{10}{31}$
Mean tone. Negative perfect thirds.	...	$-\dfrac{1}{29\cdot227}$	$-\dfrac{4}{29\cdot227}$	$-\dfrac{10}{29\cdot227}$
50	-2	$-\dfrac{1}{25}$	$-\dfrac{4}{25}$	$-\dfrac{10}{25}$
19	-1	$-\dfrac{1}{19}$	$-\dfrac{4}{19}$	$-\dfrac{10}{19}$

Departure of perfect fifth = ·01955 = $\dfrac{1}{51\cdot151}$

Departure of perfect third = −·13686 = $-\dfrac{8}{58\cdot454}$

The above Table exhibits the mode of derivation of the departures of the approximate fifths, thirds, and harmonic

sevenths of the principal systems. The actual values in deci-
mals are easily deduced ; but those here given exhibit the rela-
tions better.   (For a Table of the actual values I must refer
to my paper on Temperament in the current volume of the
Royal Society's ' Proceedings,' or to the paper in the ' Transac-
tions ' of the Musical Association.)   The values for the perfect
harmonic seventh are

$$- \frac{14}{44 \cdot 91} = - \frac{10}{32 \cdot 084} = - \cdot 31174 ;$$

whence the system of 41 would represent it best of the positive
systems enumerated, and the system of 31 best of the negative
systems ; the latter representation is rather close.

Symmetrical Arrangement and Generalized Key-board.

For an illustration of the application of the principle of sym-
metrical arrangement to positive systems, and for a woodcut of
the generalized key-board, I must refer to my papers in the Royal
Society's ' Proceedings ' and the ' Transactions ' of the Musical
Association.

Any interval formed in any regular system can be represented
by an expression of the form $x + y\delta$, where x is an integral num-
ber of E.T. semitones, and y the number of fifths whose depar-
ture is involved.   The principle of symmetrical arrangement
which I adopt consists in representing notes by positions deter-
mined by x as an abscissa and y as an ordinate, where x and y
have integral values.   The second term is always taken positive
for fifths up, and negative for fifths down, whatever be the sign
of the departure δ.   The reason for this is the convenience of
having the same key-board for positive and negative systems.
The best mode of describing the key-board will probably be to
refer the middle points of the ends of the keys to three rectan-
gular axes, x being measured along the length of the key-board,
y along the depth (*from* the player), and z vertically upwards.
The note c on the left nearest the player may be taken for
origin.   The numbers of units in y and z are the same.   The
extremities of the keys all lie in a plane rising like the slope of
a desk at an angle $\tan^{-1} 1\frac{1}{3}$.   The values of x correspond to half-
inches, of y to quarter-inches, and of z to twelfths of an inch.
Thus, for the departure of 12 fifths, $x = 0$, $y = 3$ inches, $z = 1$
inch ; that is, the key $\diagup c$ is straight behind and a little above c,
3 inches back and 1 inch up.   The octave is made 6 inches wide
($x = 12$).   In ordinary keyboards this width is $6\frac{1}{2}$ inches.   The
middle lines of the columns of keys are $\frac{1}{2}$ an inch apart, and
the keys themselves $\frac{3}{8}$ inch broad ; so that there is $\frac{5}{8}$ inch alto-
gether between the two keys which rise on each side of any

given key. This is of importance; *e. g.* in the chord $c-\searrow e-g-c$ taken with the right hand, the first finger has to reach $\searrow e$ between the adjoining keys $e\flat-f$, and a little under the *e*. All the keys are cut away so as to overhang about $\frac{1}{2}$ an inch for this purpose; so that though the length of each key on a plan is only 3 inches, yet the tangible length is $3\frac{1}{2}$ inches. Notes derived from the black keys of the E.T. (ordinary) key-board are black ($x = 1, 3, 6, 8, 10$). The other keys, corresponding to $x = 0, 2, 4, 5, 7, 9, 11$, are white.

It results from the arrangement by intervals that all combinations, scales, or chords of any kind have the same form for the finger in whatever key they are taken, a simplification which gives the learner one thing to acquire where there are twelve on the ordinary key-board.

The thirds used in positive and negative systems being differently derived, the resulting forms of scale on the key-board are different. The positive scales can be played with a little practice; and all desirable chords and combinations can be grasped with ease. But as far as facility of performance goes, the negative systems afford the principal simplification. I believe I am not exaggerating in saying that it may be possible to execute in this manner passages of greater complexity and difficulty than can be performed on the ordinary key-board. The movements of the fingers required are less in amount and simpler in character. Passages of the most rapid kind are performed with ease. The mean-tone stop on my small enharmonic organ has given me the opportunity of realizing what I had always anticipated in this respect.

Practical Applications.

The first instrument constructed with generalized key-board was a large harmonium. The key-board has 84 keys to each octave; it was thus arranged in order that, in the experiment with the system of 53 in particular, there might be no risk of failure from any deficiency in the extent of the instrument. I specially wish to direct attention to the fact that both the instruments hitherto constructed are designed for experimental purposes—as an objection constantly made against my instruments is, that if put into a practical form their bulk would be enormous. When a practical instrument is to be constructed, the question will be, what is the least extent of development that will answer? hitherto, for the experimental instruments, it has been, what is the largest development that may possibly be needed? For instance, the only practical instrument I clearly see my way to at present is one for some kind of negative system. My mean-tone stop has 36 notes to the octave; but as

a result of experience, I am satisfied that 24 will be sufficient for all practical purposes. Probably Smith's system of fifths and thirds beating equally in opposite directions would be best to begin with.

The large harmonium was tuned according to the division of the octave into 53 equal intervals. For the details of the tuning and the specialization of the notation in the system of 53, I must refer to the 'Transactions' of the Musical Association. The compass of the key-board is $4\frac{1}{2}$ octaves; and the keenness of the tone, the perfect standing in tune, and the general completeness of the instrument have enabled me, during the two years it has been in my possession, to obtain a considerable familiarity with one of the most important of the approximately just systems.

The fifths of the system of 53 being practically perfect, the thirds by 4 fifths up are practically Pythagorean thirds. The contrast between these and the approximately perfect thirds may be called startling.

With the view of having an instrument which should be less troublesome to take to pieces for removal, and also with a view to obtain a negative system on the key-board, a small organ was subsequently constructed with a generalized key-board of three octaves in compass, 48 keys per octave and two stops, the one tuned to the positive system of perfect thirds, and the other to the mean-tone system. The mean-tone system has only 36 pipes per octave. The pipes are metal stopped diapasons; and their accurate tuning is effected by stoppers of a peculiar construction, containing an internal slider worked by a projecting screw, which may be regarded as a fine adjustment. The arrangement is of a general character, which is susceptible of extension; and all the working parts can be got at quite easily, which is not the case with the harmonium. This organ, as well as my tuning-machine, the great harmonium, and an ordinary organ, have been built for me by Mr. T. A. Jennings, whose exceptional mechanical skill alone has enabled me to prosecute these investigations with any success.

For any further details I am obliged to refer to the two papers above quoted. I have here endeavoured to give a general summary of my investigations, avoiding technical details. The points I specially wish to direct attention to are:—the question of melodic sequences; the unsuitability of just systems for ordinary music, and the desirability of writing specially with a view to their employment; and the suitability of negative systems for ordinary music; but especially it is most desirable that it should be felt how worthless theoretical ideas about the properties of musical sounds are when unaccompanied by practical study.

XXII. *On the Flexure of Continuous Girders.* By MANSFIELD
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field Scientific School, New Haven, Conn., U. S. A.**

IN the ordinary text-books on Mechanics the theory of the
flexure of girders is developed for particular cases. First
a beam fastened at one end and supporting a weight at the other
is considered, and the equation of the elastic line for that case is
applied to girders of one span with free ends. Then, by methods
tedious and particular, beams with fixed ends are treated ; and
occasionally may be found investigations upon girders of two
spans for special cases of loading, such as a uniform load over
the whole of the girder, or a single load in the middle of one of
the spans. The impression is conveyed that the investigation
of continuous girders is too difficult to be undertaken except when
the load in each span is uniformly distributed over its entire
length. The student who is interested in this branch of mathe-
matical analysis and who seeks to extend his investigations, finds
but little to assist his progress, and is apt to drop the subject in
despair, particularly as he finds authorities hinting that the theory
is too complicated for development†.

I propose in this article to demonstrate and present a few new
and simple formulæ which shall include the whole theory of the
moments, shearing-forces, and reactions for girders of any num-
bers of spans, equal or unequal, subject to any assignable loads.
The remarkable theorem of three moments for concentrated
weights will serve as a basis for my investigation ; and as I am
unaware of any work in English which presents a proof of that
theorem, I shall first give an abridged demonstration of it. The
formulæ that I shall deduce from it will be found to be perfectly
general, and yet in a form easily applied to any particular case,
as the examples which follow them will illustrate. Armed with
these algebraic expressions, which may be written upon a sheet
of common note-paper, the student may attack and solve every
problem relating to the flexure of girders over level supports.

The following are the conditions which enable the theory of
continuous girders to be mathematically investigated :—(1) the
extensions and compressions of different fibres are proportional
to their distances from the neutral axis ; (2) the deflection is
small compared with the length of the beam ; (3) the moment
of inertia is constant. In the following pages the supports will
also be considered upon the same level.

The curve which a beam assumes under the action of its own
weight and the loads that it supports is known as the elastic

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* Communicated by the Author.
† For example, Humber, ‘ Strains in Girders,’ art. 31.

line.   From the first two hypotheses stated above its differential equation is easily derived and given in all books on the theory of beams.   This equation is

$$\frac{d^2y}{dx^2} = \frac{m}{\text{EI}},$$

m being the moment of the molecular forces in the section whose abscissa is x, E the modulus of elasticity, and I the moment of inertia of the beam.   To obtain from this the equation for any particular case, it is only necessary to substitute for m and I their values in terms of x and integrate the equation twice.   In what follows, I will be regarded as constant.   Let $o\,q$ represent
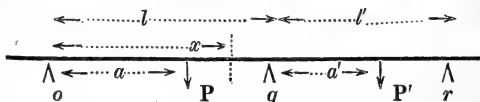
a span of a continuous girder with level supports whose length is l.   Let P be a single concentrated load at a distance a from the support o; also let the span $q\,r$ be equal to l', and the load P′ be placed at a distance a' from the support q.   Let M, M′, and M″ denote the moments at the supports o, q, and r respectively.   Then all the exterior forces which act upon the beam to the left of the point o may be replaced by the horizontal couple M and a vertical shearing-force S.   Since equilibrium prevails, we have for a section between P and q the equation of moments

$$\text{M} - \text{S}x + \text{P}(x-a) - m = 0. \quad . \quad . \quad . \quad \text{(I.)}$$

Making in this $x = l$, m becomes M′, and we have

$$\text{S} = \frac{\text{M} - \text{M}'}{l} + \frac{\text{P}(l-a)}{l} = \frac{\text{M} - \text{M}'}{l} + \text{P}(1-k), \quad . \quad \text{(I′.)}$$

a being replaced by kl, where k denotes any fraction.   Insert now the value of m in the differential equation of the curve, and integrate it twice.   The constant for the first integration is t, the tangent of the angle which the curve at the origin makes with the axis of abscissæ; the constant for the second integration is zero; then the required equation is

$$y = tx + \frac{1}{6\text{EI}}[3\text{M}x^2 - \text{S}x^3 + \text{P}(x-a)^3]. \quad . \quad . \quad \text{(II.)}$$

Insert in this the value S in terms of M, M′, and P; also make $x = l$ and put $a = kl$; then $y = 0$, and we get the expression

$$6\text{EI}t = -2\text{M}l - \text{M}'l + \text{P}l^2(2k - 3k^2 + k^3).$$

Now in the value of $\frac{dy}{dx}$ make $x = l$; $\frac{dy}{dx}$ then becomes t', the tan-

gent at q, and we have

$$6EIt' = Ml + 2M'l - Pl^2(k - k^3). \quad . \quad . \quad . \quad \text{(III.)}$$

If we consider the origin at q we can also find another expression for t', depending on M', M'', and P' ; this is of course analogous to the value of t written above, or

$$6EIt' = -2M'l' - M''l' + P'l'^2(2k - 3k^2 + k^3), \quad . \quad \text{(III'.)}$$

where k denotes $\dfrac{a'}{l'}$, and is not necessarily the same in the two expressions. Comparing then the two values of t', we get the *Theorem of Three Moments* for concentrated loads, or

$$Ml + 2M'(l + l') + M''l' = Pl^2(k - k^3) + P'l'^2(2k - 3k^2 + k^3). \quad \text{(IV.)}$$

If there be many loads, we have only to prefix to the terms involving P and P' the sign of summation Σ. For uniformly distributed loads w and w' per unit of length, we place $\Sigma P = \int wd(kl)$ and $\Sigma P' = \int w'd(kl')$ and integrate between the required limits. If the loads extend over the whole span, the first integral is taken between $kl = 0$ and $kl = l$, the second between $kl' = 0$ and $kl' = l'$. Then

$$Ml + 2M'(l + l') + M''l' = \tfrac{1}{4}wl^3 + \tfrac{1}{4}w'l'^3, \quad . \quad . \quad \text{(V.)}$$

which is the theorem as first deduced by Clapeyron[*].

With the above formulæ as a basis, I propose to develop two expressions by which the moment at any support can be directly and easily determined without the application of the theorem of three moments, and which are in so simple a form that their use is far preferable to the tedious solution of the equations arising in the process as ordinarily followed.

Remembering that all girders to be investigated are subject to the four conditions mentioned above, they may be regarded for the purposes of this article as forming three classes. The first class includes all girders whose two ends rest free upon the abutments, the second where one end is resting and the other walled in or fastened horizontally, and the third where both ends are fixed horizontally.

CASE I. *Ends resting free upon abutments.*
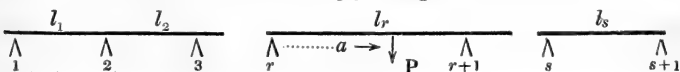
Let the girder consist of any number of unequal spans, the

* *Comptes Rendus*, 1857. The extension to concentrated loads was made by Bresse and Winkler independently about 1863. Vide *La Mécanique Appliquée* and *Die Elasticitäts-Lehre*.

rth span only being loaded. Let $s =$ the number of spans, and l_1, l_2, l_r, &c. the lengths of the spans, the indices counting from the left; the index n will refer to any support. A single load in the rth span is called P, and its distance from the rth supports is kl_r or a. Referring to (IV.) we see that there will be two functions of P and kl_r that are of frequent occurrence: the one corresponding to the equation for the support r is denoted by A; the other, for the support $r+1$, by B. Then

$$\left.\begin{aligned} \mathrm{A} &= \mathrm{P}l_r^2(2k-3k^2+k^3) \\ \mathrm{B} &= \mathrm{P}l_r^2(k-k^3), \end{aligned}\right\} \text{ for a single load in the } r\text{th span.}$$

The uniformly distributed load in l_r is w per unit of length; then, from what is said under (IV.), we have

$$\left.\begin{aligned} \mathrm{A} &= \int_{k_1}^{k_2} wl_r^3(2k-3k^2+k^3)dk, \\ \mathrm{B} &= \int_{k_1}^{k_2} wl_r^3(k-k^3)dk, \end{aligned}\right\} \begin{aligned} &\text{for a uniform load whose ends} \\ &\text{are distant } k_1l_r \text{ and } k_2l_r \text{ from} \\ &\text{the support } r. \end{aligned}$$

If the uniform load cover the whole span, $k_1 = 0$ and $k_2 = 1$; then

$$\mathrm{A} = \mathrm{B} = \tfrac{1}{4}wl_r^3 \text{ for a uniform load over } l_r.$$

The equations of moments for girders loaded only in the span l_r are, from (IV.), since the moments at the abutments are zero,

$$2\mathrm{M}_2(l_1+l_2) + \mathrm{M}_3 l_2 = 0,$$
$$\mathrm{M}_2 l_2 + 2\mathrm{M}_3(l_2+l_3) + \mathrm{M}_4 l_3 = 0,$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\mathrm{M}_{r-1} l_{r-1} + 2\mathrm{M}_r(l_{r-1}+l_r) + \mathrm{M}_{r+1} l_r = \mathrm{A},$$
$$\mathrm{M}_r l_r + 2\mathrm{M}_{r+1}(l_r+l_{r+1}) + \mathrm{M}_{r+2} l_{r+1} = \mathrm{B},$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$\mathrm{M}_{s-2} l_{s-2} + 2\mathrm{M}_{s-1}(l_{s-2}+l_{s-1}) + \mathrm{M}_s l_{s-1} = 0,$$
$$\mathrm{M}_{s-1} l_{s-1} + 2\mathrm{M}_s(l_{s-1}+l_s) = 0.$$

The solution of these equations is best effected by the method of indeterminate multipliers. Let the first equation be multiplied by c_2, the second by c_3, &c., the index of the indeterminate numbers corresponding with that of M in the middle term. Then let all the equations be added and the coefficients of M_2, M_3, &c. be combined; then we have

$$\mathrm{M}_2[2c_2(l_1+l_2) + c_3 l_2] + \mathrm{M}_3[c_2 l_2 + 2c_3(l_2+l_3) + c_4 l_3] + \ldots$$
$$+ \mathrm{M}_r[c_{r-1} l_{r-1} + 2c_r(l_{r-1}+l_r) + c_{r+1} l_r] + \ldots$$
$$+ \mathrm{M}_s[c_{s-1} l_{s-1} + 2c_s(l_{s-1}+l_s)] = \mathrm{A}c_r + \mathrm{B}c_{r+1}.$$

Now let such relations exist between the multipliers that all the

terms in the first member shall reduce to zero except the last containing M_s, then the value of M_s is

$$M_s = \frac{Ac_r + Bc_{r+1}}{c_{s-1}l_{s-1} + 2c_s(l_{s-1} + l_s)};$$

and the values of the multipliers are given by the equations

$$2c_2(l_1 + l_2) + c_3 l_2 = 0,$$

$$c_2 l_2 + 2c_3(l_2 + l_3) + c_4 l_3 = 0,$$

$$\cdot \qquad \cdot \qquad \cdot$$

$$c_{r-1} l_{r-1} + 2c_r(l_{r-1} + l_r) + c_{r+1} l_r = 0,$$

$$\cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot$$

After deducing the values of c from these equations, the value of M_s then becomes known.

Now, if we multiply the equations of moments, beginning with the last, by d_2, d_3, &c., all the moments except M_2 may be eliminated, and we have

$$M_2 = \frac{Ad_{s-r+2} + Bd_{s-r+1}}{d_{s-1}l_2 + 2d_s(l_1 + l_2)},$$

and the multipliers will be given by

$$2d_2(l_s + l_{s-1}) + d_3 l_{s-1} = 0,$$

$$d_2 l_{s-1} + 2d_3(l_{s-1} + l_{s-2}) + d_4 l_{s-2} = 0,$$

$$\cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot$$

The values of the indeterminate numbers need only fulfil the condition that they satisfy the equations as given above. Assuming then $c_2 = 1$ and $d_2 = 1$, we get the following values :—

$$
\left.
\begin{aligned}
&c_1 = 0, && d_1 = 0, \\
&c_2 = 1, && d_2 = 1, \\
&c_3 = -2\,\frac{l_1 + l_2}{l_2}, && d_3 = -2\,\frac{l_s + l_{s-1}}{l_{s-1}}, \\
&c_4 = -2c_3\,\frac{l_2 + l_3}{l_3} - c_2\,\frac{l_2}{l_3}, && d_4 = -2d_3\,\frac{l_{s-1} + l_{s-2}}{l_{s-2}} - d_2\,\frac{l_{s-1}}{l_{s-2}}, \\
&c_5 = -2c_4\,\frac{l_3 + l_4}{l_4} - c_3\,\frac{l_3}{l_4}, && d_5 = -2d_4\,\frac{l_{s-2} + l_{s-3}}{l_{s-3}} - d_3\,\frac{l_{s-2}}{l_{s-3}}, \\
&\quad\cdot\quad\cdot\quad\cdot\quad\cdot && \quad\cdot\quad\cdot\quad\cdot\quad\cdot\quad\cdot
\end{aligned}
\right\} \quad . \; (1)
$$

Since the equations of moments are of the same form as the equations of the multipliers, we have

$$M_3 = c_3 M_2, \quad M_4 = c_4 M_2,$$

or, universally,

$$\text{when } n < r+1, \ \ M_n = c_n M_2 = c_n \frac{A d_{s-r+2} + B d_{s-r+1}}{d_{s-1} l_2 + 2 d_s (l_1 + l_2)}; \quad . \quad (2)$$

also

$$M_{s-1} = d_3 M_s, \quad M_{s-2} = d_4 M_s, \text{ &c.,}$$

or,

$$\text{when } n > r, \ \ M_n = d_{s-n+2} M_s = d_{s-n+2} \frac{A c_r + B c_{r+1}}{c_{s-1} l_{s-1} + 2 c_s (l_{s-1} + l_s)}. \ (3)$$

From (1), (2), and (3) we may obtain the moments at every support due to a load in the span l_r. These once found, it is easy to get the shearing-forces and reactions. Referring to (I'.), we see that the expression for the shear in the span l_r at a point infinitely near to the rth support depends upon the moments M_r and M_{r+1} and the quantity $P(1-k)$. By exactly the same reasoning we may show that the shear in the span l_r at a point infinitely near to the $r+1$th support depends upon the difference of the moments M_{r+1} and M_r and the quantity Pk. Let these quantities be represented by a and b, then

$$\left. \begin{array}{l} a = P(1-k), \\ b = Pk, \end{array} \right\} \text{for a single load};$$

$$\left. \begin{array}{l} a = \displaystyle\int_{k_1}^{k_2} w l_r (1-k)\, dk, \\ b = \displaystyle\int_{k_1}^{k_2} w l_r k\, dk, \end{array} \right\} \begin{array}{l} \text{for a uniform load whose ends are} \\ \text{limited by the abscissæ } k_1 l_r \text{ and } k_2 l_r; \end{array}$$

$a = b = \frac{1}{2} w l_r$ for a uniform load over the whole span l_r.

Let the shear in the span l_r at a point infinitely near to the rth support be denoted by S_r, and the shear infinitely near to the $r+1$th support by S'_r; then

$$
\left.
\begin{array}{ll}
S_r &= \dfrac{M_r - M_{r+1}}{l_r} + a \left\{ \begin{array}{l} \text{for the right-hand shear at the } r\text{th} \\ \text{support,} \end{array} \right. \\[2ex]
S'_r &= \dfrac{M_{r+1} - M_r}{l_r} + b \left\{ \begin{array}{l} \text{for the left-hand shear at the } r+1\text{th} \\ \text{support,} \end{array} \right. \\[2ex]
S_n &= \dfrac{M_n - M_{n+1}}{l_n} \left\{ \begin{array}{l} \text{for the right-hand shear at all supports} \\ \text{except } r, \end{array} \right. \\[2ex]
S'_{n-1} &= \dfrac{M_n - M_{n-1}}{l_{n-1}} \left\{ \begin{array}{l} \text{for the left-hand shear at all supports} \\ \text{except } r+1; \end{array} \right.
\end{array}
\right\} \quad (4)
$$

then the reaction at any support is

$$R_n = S'_{n-1} + S_n, \quad R_r = S'_{r-1} + S_r \ \text{ &c.} \quad . \quad . \quad (5)$$

Let m and s denote the moment and shearing-force at any section; then from (I.)

$$m = M_r - S_r x + P(x-a) \text{ for a section between P}$$
$$\text{and the following support,} \qquad (6)$$
$$m = M_n - S_n x \text{ for any other section.}$$

Also we have

$$s = P - S_r \text{ for a section between P and the } r+1\text{th}$$
$$\text{support,} \qquad (7)$$
$$s = -S_n \text{ for any other section.}$$

Equations (6) and (7) refer to a concentrated load; for a uniform load we have only to put $P = \int w\,da$, as in the other cases.

CASE II. *One end free, the other fastened horizontally.*
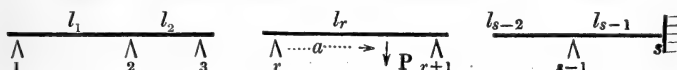
If in (1) to (3) we make $l_s = 0$, it is evident that the elastic line passes through two consecutive points, and hence the tangent at that point is horizontal; therefore for this case we have the following rule :—Make $l_s = 0$, and let $s-1 =$ number of spans; then formulæ (1) to (7) are directly applicable.

CASE III. *Both ends fastened horizontally.*
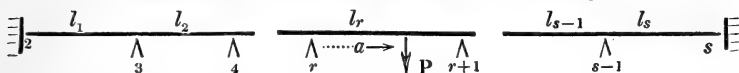
Let the indices be placed as in the figure; then in formulæ (1) to (7) make $l_1 = 0$ and $l_s = 0$, and let $s-2 =$ number of spans.

From the above equations can be easily solved all the exercises given in text-books for girders of one span, and also thousands of interesting problems for beams of many spans; (1) to (7) give the moments and shears at every cross section. Making $m = 0$ in (6), the value of x gives the place of the inflection-points. The deflection at any point x is given by y from (II.); and its maximum value is obtained by the usual methods. The following examples have been chosen to illustrate the use of the formulæ.

1. In a girder of four *equal* spans with free ends, to find the moments at every support due to a single load in the centre of one of the end spans.
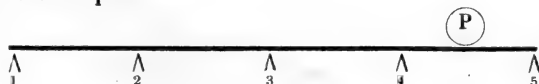
Let $l=$ the length of each span; also $s=4$ and $r=4$. First substitute in (1) for l_1, l_2, l_s, &c. the value l; then

$$c_1=d_1= \quad 0, \qquad\qquad c_6=d_6= \quad 209,$$
$$c_2=d_2= \quad 1, \qquad\qquad c_7=d_7=- \quad 780,$$
$$c_3=d_3=- \quad 4, \qquad\qquad c_8=d_8= \quad 2911,$$
$$c_4=d_4= \quad 15, \qquad\qquad c_9=d_9=-10864,$$
$$c_5=d_5=-56, \qquad\qquad c_{10}=-4c_9-c_8 \text{ &c.*}$$

Then from (2) we have,

$$\text{when } n<5, \ \ \mathrm{M}_n=c_n\frac{\mathrm{A}d_2+\mathrm{B}d_1}{l(d_3+4d_4)}=\frac{c_n\mathrm{P}l(2k-3k^2+k^3)}{56}.$$

Placing $k=\frac{1}{2}$, we get,

$$\text{when } n>5, \ \ \mathrm{M}_n=\frac{3c_n\mathrm{P}l}{448}.$$

Making $n=1, 2, 3$, and 4 successively, we find

$$\mathrm{M}_1=0, \quad \mathrm{M}_2=\frac{3\mathrm{P}l}{448}, \quad \mathrm{M}_3=-\frac{12\mathrm{P}l}{448}, \text{ and } \mathrm{M}_4=\frac{45\mathrm{P}l}{448}.$$

M_5 is of course equal to zero. This is also seen from (3); for making $n=s+1$, $d_{s-n+2}=d_1=0$, hence $\mathrm{M}_5=0$.

2. In a girder of three spans with free ends, the length of the centre one being l, and of the two end ones equal to αl, to find the reactions at the abutments due to a single load in one of the end spans.

$$\frac{a}{l_r}=\frac{a}{\alpha l}=k.$$
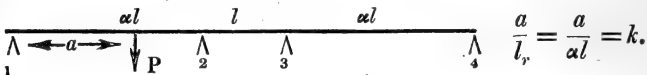
From (1) we have

$$c_1=d_1= \quad 0,$$
$$c_2=d_2= \quad 1,$$
$$c_3=d_3=-(2+2\alpha).$$

Since $s=3$ and $r=1$, equation (3) becomes,

$$\text{when } n>1, \ \ \mathrm{M}_n=d_{5-n}\frac{\mathrm{A}c_1+\mathrm{B}c_2}{l(c_2+2(1+\alpha)c_3)}=d_{s-n}\frac{\mathrm{B}}{-l(3+8\alpha+4\alpha^2)}$$

$$=d_{5-n}\frac{\mathrm{P}l\alpha^2(k-k^3)}{-(3+8\alpha+4\alpha^2)}.$$

* Only four of these are needed for the solution of the problem. They are known as the Clapeyronian numbers, from the name of their discoverer, and are hence appropriately designated by the letter c.

Making $n=2$ and $n=3$, we get

$$M_2 = \frac{2+2\alpha}{3+8\alpha+4\alpha^2} P\alpha^2 l(k-k^3) \text{ and } M_3 = -\frac{P\alpha^2 l(k-k^3)}{3+8\alpha+4\alpha^2}.$$

Now from (4) and (5)

$$R_1 = -\frac{M_2}{\alpha l} + P(1-k) = \frac{P}{\Delta}[\Delta - (\Delta + 2\alpha + 2\alpha^2)k + (2\alpha + 2\alpha^2)k^3],$$

$$R_4 = -\frac{M_3}{\alpha l} = \frac{P}{\Delta}[\alpha k - \alpha k^3],$$

where Δ denotes the quantity $3+8\alpha+4\alpha^2$.

Knowing α, we can find the reactions for every value of k. For instance, let $\alpha = 2$; then $\Delta = 35$, and

$$R_1 = P(1 - 1\cdot3428\,k + 0\cdot3428\,k^3),$$
$$R_4 = 0\cdot0571\,(k - k^3).$$

For $k = \frac{1}{2}$,

$$R_1 = 2\cdot972\,P \text{ and } R_4 = 0\cdot0214\,P.$$

3. In a girder of two unequal spans, ends free, to find the reaction at the pier due to a uniform load over a part of one of the spans.
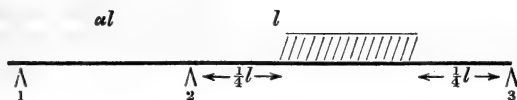
Let one span equal l, the other αl, and let the uniform load be placed between the limits $k_1 = \frac{1}{4}$, $k_2 = \frac{3}{4}$; then we have

$$A = \tfrac{11}{64} w l^3, \quad a = \tfrac{1}{4} w l.$$

In this case $s = 2$ and $r = 2$; hence from (2),

$$M_2 = c_2 \frac{Ad_2 + Bd_1}{2l(\alpha+1)d_2} = \frac{A}{2l(\alpha+1)} = \frac{11wl^2}{128(1+\alpha)}.$$

Then, from (6) and (7),

$$R_2 = \frac{M_2}{\alpha l} + \frac{M_2}{l} + a = \frac{11wl}{128\alpha} + \frac{wl}{4} = \frac{(11+32\alpha)wl}{128\alpha}.$$

Let $\alpha = \frac{1}{2}$, then

$$R_2 = \frac{27}{64} wl.$$

Suppose this same load to be concentrated at its middle point. Here we have $P = \frac{1}{2} wl$ and $k = \frac{1}{2}$; hence $A = \frac{3}{16} wl^3$ and $a = \frac{1}{4} wl$.

Then, as before,

$$M_2 = \frac{A}{2l(1+\alpha)} = \frac{3wl^2}{32(1+\alpha)} \quad \text{and} \quad R_2 = \frac{(3+8\alpha)wl}{32\alpha};$$

and when $\alpha = \frac{1}{2}$,

$$R_2 = \frac{7}{16}wl = \frac{28}{64}wl.$$

4. In a girder of many equal spans, loaded in the last, to find the inflection-points in the unloaded spans, ends being free. In (6) make $m = 0$; then

$$x_n = \frac{M_n}{S_n} = \frac{M_n l}{M_n - M_{n+1}} = \frac{c_n M_2 l}{c_n M_2 - c_{n+1} M_2} = \frac{c_n l}{c_n - c_{n+1}}.$$

Making m equal to 1, 2, 3, &c., we find as the distance of the inflection-points from the 1st, 2nd, 3rd, &c. supports (the values of c are given under problem 1),

$$x_1 = 0, \quad x_2 = \frac{l}{5}, \quad x_3 = \frac{4l}{19}, \quad x_5 = \frac{15l}{71}, \quad x_6 = \frac{56l}{265}, \text{&c.,}$$

where we see that both numerators and denominators follow the law of Clapeyron's numbers ; *i. e.* any one is equal to one fourth the sum of the preceding and following.

5. In a beam over four supports, one end free and the other fixed, to find the maximum moments in the middle span due to a uniform load in that span.
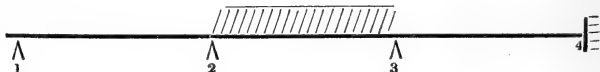
Let all the spans be equal ; $s - 1 =$ number of spans, hence $s = 4$; then in (I.) make $l = 0$, and we have

$c_1 = \quad 0,$	$d_1 = \quad 0,$
$c_2 = \quad 1,$	$d_2 = \quad 1,$
$c_3 = -\ 4,$	$d_3 = -2,$
$c_4 = \quad 15,$	$d_4 = \quad 7.$

Also $r = 2$. Hence, from (2) and (3),

$$\text{when } n < 3, \quad M_n = c_n \frac{Ad_4 + Bd_3}{l(d_3 + 4d_4)} = c_n \frac{5wl^3}{104};$$

$$\text{when } n > 2, \quad M_n = d_{6-n} \frac{Ac_2 + Bc_3}{l(c_3 + 2c_4)} = -c_n \frac{3wl^2}{104}.$$

Therefore

$$M_1 = 0, \quad M_2 = \frac{5wl^2}{104}, \quad M_3 = \frac{6wl^2}{104}, \quad M_4 = -\frac{3wl^2}{104}.$$

M_2 and M_3 are the greatest positive moments in the middle span; to find the maximum negative moment, we take (6),

$$m = M_2 - S_2 x + \frac{wx^2}{2};$$

this becomes a maximum for $x = \dfrac{S_2}{w}$; inserting this in the expression for m, we get

$$m = M_2 - \frac{S_2^2}{w} + \frac{S_2^2}{2w} = M_2 - \frac{S_2^2}{2w}.$$

Now, from (4),

$$S_2 = \frac{M_2 - M_3}{l} + \frac{1}{2} wl = \frac{51}{104} wl.$$

Therefore the greatest negative moment is

$$m = -\frac{1561 \, wl^2}{21632} = -\frac{7\frac{1}{2} wl^2}{104};$$

and it obtains at the point where $x = \dfrac{S_2}{w} = \dfrac{51}{104} l.$

6. In a girder of two equal spans, ends fastened horizontally, to find the reactions and inflection-points due to a single load.
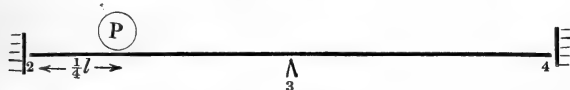
Let the load be placed in the first span, then $r = 2$; since $s - 2$ = number of spans, $s = 4$; then from (1),

$$\begin{aligned} c_1 &= d_1 = 0, \\ c_2 &= d_2 = 1, \\ c_3 &= d_3 = -2, \\ c_4 &= d_4 = 7. \end{aligned}$$

From (2) and (3) we have,

when $n < 3$, $M_n = c_n \dfrac{A d_4 + B d_3}{l(d_3 + 2d_4)} = c_n \dfrac{Pl}{4} (4k - 7k^2 + 3k^3);$

when $n > 2$, $M_n = c_{6-n} \dfrac{A c_2 + B c_3}{l(c_3 + 2c_4)} = c_{6-n} \dfrac{Pl}{4} (k^2 - k^3).$

Let the load be placed at a distance from the abutment equal to $\frac{1}{4} l$, then $k = \frac{1}{4}$; making also n equal to 2, 3, and 4, we get

$$M_2 = \frac{39 Pl}{256}, \quad M_3 = \frac{6 Pl}{256}, \quad \text{and } M_4 = -\frac{3 Pl}{256}.$$

Then, from (4),

$$S_2 = \frac{M_2 - M_3}{l} + P(1-k) = \frac{225\,P}{256},$$

$$S_2' = \frac{M_3 - M_2}{l} + Pk = \frac{31\,P}{256}, \quad S_2 = \frac{M_3 - M_4}{l} = \frac{9\,P}{256},$$

$$S_3' = \frac{M_4 - M_3}{l} = -\frac{9\,P}{256};$$

and from (5),

$$R_2 = S_2 = \frac{225\,P}{256}, \quad R_3 = S_2' + S_3 = \frac{40\,P}{256}, \quad R_4 = S_3' = -\frac{9\,P}{256}.$$

For the inflection-point in the unloaded span, we have from (6),

$$M_3 - S_3 x_3 = 0 \text{ or } x_3 = \frac{M_3}{S_3} = \frac{2}{3}\,l;$$

for the one between the abutment and P,

$$x_1 = \frac{M_2}{S_2} = \frac{39}{225}\,l = 0.173\,l;$$

for the one between P and the pier,

$$x_2 = \frac{M_2 - Pa}{S_2 - P} = \frac{25}{31}\,l = 0.806\,l.$$

7. To find the greatest deflections in both spans of the girder in the last problem.

The equation of the elastic curve between P and the pier is, from (II.) (since the tangent at the fixed end is horizontal, $t = 0$),

$$6EI\,y = M_2 x^2 - S_2 a^3 + P(x - a)^3.$$

Substituting the values of M_2, S_2, and a, and differentiating, we find y to be a maximum when $x = 0.413\,l$; substituting this, we have

$$y = 0.0034\,\frac{Pl^3}{EI} \text{ for the value of the maximum deflection.}$$

The equation of the curve for the unloaded span is

$$6EI\,y = 6EI\,t_3 x + M_3 x - S_3 x,$$

where t_3 is given by (III.) or (III'.), or

$$6EI\,t_3 = -2M_3 l - M_4 l = -9Pl^2.$$

Substituting t_3, M_3, and S_3, we find that y becomes a negative maximum for $x = \frac{1}{3}\,l$; then from the equation of the curve we get

$$y = -0.0009\,\frac{Pl^3}{EI} \text{ as the minimum deflection.}$$

If a girder have loads in several spans, we find the moments due to the load in each span separately, and add the results. It will thus be seen that the formulæ (1) to (7), in connexion with the equation of the elastic line, contain in a form easy for use the whole theory of continuous girders over level supports.

XXIII. *On the Theorem of the Mean Ergal, and its Application to the Molecular Motions of Gases.* By R. CLAUSIUS.

[Concluded from p. 117.]

§ 15. THE quantities $\mathfrak{u}_1$, $\mathfrak{u}_2$, $\mathfrak{u}_3$ in equation (94) we will first subject to a closer consideration.

According to equation (91),

$$\log \mathfrak{u} = \int_0^\infty \log u \cdot z^2 f(z)\, dz.$$

Here u has the meaning given in (86), namely

$$u = m \overline{\left(\frac{dx}{dt}\right)^2} i^2.$$

Now, as $\left(\frac{dx}{dt}\right)^2$ is constant for most of the time and only during the brief periods of collision has different values, $i \sqrt{\overline{\left(\frac{dx}{dt}\right)^2}}$ is approximately equal to the projection, referred to the x-direction, of the path travelled by the point during the time i; and as, further, the point during this time runs once forwards and back again between the two walls perpendicular to the x-direction, $i \sqrt{\overline{\left(\frac{dx}{dt}\right)^2}}$ is approximately equal to $2(c + c')$, and consequently u approximately $= 4m(c + c')^2$. This holds for all the points, notwithstanding the inequality of z; and if we take account of this in the above formula for $\log \mathfrak{u}$, and recollect that

$$\int_0^\infty z^2 f(z)\, dz = 1,$$

we recognize that $\log \mathfrak{u}$ must be nearly equal to $\log 4m(c + c')^2$. The latter result we will bring into a form more convenient for what follows, by saying that $\sqrt{\dfrac{\mathfrak{u}}{4m}}$ is nearly equal to $c + c'$.

This result, found for any coordinate-direction, we can of course express also for the three directions of the coordinates singly,

employing for distinction the indices 1, 2, and 3. The logarithm which arises from the addition of the three logarithms we will represent by a simplified symbol, putting

$$\log \mathfrak{U} = \tfrac{1}{2} \log \frac{\mathfrak{u}_1 \mathfrak{u}_2 \mathfrak{u}_3}{(4m)^3},$$

or

$$\mathfrak{U} = \sqrt{\frac{\mathfrak{u}_1 \mathfrak{u}_2 \mathfrak{u}_3}{(4m)^3}}. \quad . \quad . \quad . \quad . \quad (95)$$

The quantity $\mathfrak{U}$ is, according to the above, approximately equal to the product $(c_1 + c'_1)(c_2 + c'_2)(c_3 + c'_3)$, or equal to the space-content of the vessel; and since the latter, according to (79) and (81), only differs by the small quantity $N\tfrac{4}{3}\pi\rho^3$ or ϵ from the volume V of the quantity of gas under consideration, which is represented by the material points present in the vessel, we may also say $\mathfrak{U}$ *differs but little from* V.

The purpose of this approximate determination of $\mathfrak{U}$ is merely to give a convenient representation of the signification of the subsequent equations. The exact determination of this quantity can likewise be effected, if beside equation (95) we take into consideration equations (91) and (86) and apply them to the before-discussed motion of the material points in the rectangularly parallelepipedal vessel of the dimensions determined by equations (81).

In consequence of equation (95) we can now put

$$\log (\mathfrak{u}_1 \mathfrak{u}_2 \mathfrak{u}_3) = 2 \log \mathfrak{U} + 3 \log (4m).$$

The last logarithm on the right-hand side is constant; so that its variation $=0$, and hence we have

$$\delta \log (\mathfrak{u}_1 \mathfrak{u}_2 \mathfrak{u}_3) = 2\delta \log \mathfrak{U},$$

by which equation (94) is transformed into

$$\delta U = \tfrac{2}{3} T \delta \log \mathfrak{U} + \Sigma \frac{dU}{dc} \delta c. \quad . \quad . \quad . \quad (96)$$

To this equation we can at once join two others, which determine the quantities E and $U-T$. For this we need only, in one case, to add, and in the other to subtract, the variation δT on both sides. We will at the same time suppose, on the right-hand side,

$$\delta T = T\delta \log T = T\delta \log \frac{T}{Nm},$$

and then, to simplify, introduce the letters $\mathfrak{E}$ and $\mathfrak{J}$ with the meanings

$$\mathfrak{E} = \mathfrak{U}\left(\frac{T}{Nm}\right)^{\frac{3}{2}}, \left.\vphantom{\frac{T}{Nm}}\right\}$$
$$\mathfrak{I} = \mathfrak{U}\left(\frac{Nm}{T}\right)^{\frac{3}{2}}. \left.\vphantom{\frac{Nm}{T}}\right\} \qquad . \quad . \quad . \quad . \quad . \quad 97)$$

The equations resulting from (96) by that addition and subtraction are then:—

$$\delta E = \tfrac{2}{3} T \delta \log \mathfrak{E} + \Sigma \frac{d U}{dc} \delta c; \quad . \quad . \quad . \quad (98)$$

$$\delta(U - T) = \tfrac{2}{3} T \delta \log \mathfrak{I} + \Sigma \frac{d U}{dc} \delta c. \quad . \quad . \quad (99)$$

We must now, finally, consider more closely the sum $\Sigma \dfrac{d U}{dc}$, which occurs in equations (98) and (99), in order to ascertain its signification.

The sign of summation refers to the six quantities c_1, c'_1, c_2, c'_2, c_3, c'_3, each of which determines the position of one of the six walls bounding the parallelepiped, and only occurs in that part of the ergal which relates to the force exerted by that wall. Calling the coordinates of a point at present x_1, x_2, and x_3, we will first consider the wall which is perpendicular to the x_1-direction and is situated on the positive side, at the distance c_1 from the origin of coordinates. The force exerted by this wall on the point along the x_1-direction will, according to our former notation, be represented by $F'(c_1 - x_1)$; and the part of the ergal referable to this force is $F(c_1 - x_1)$. Accordingly we can represent that force which conversely the point exerts on the wall by $-F'(c_1 - x_1)$ or $-\dfrac{d F(c_1 - x_1)}{dc_1}$, and, since in the course of the period it is variable, employ the symbol for its mean value:—

$$-\frac{\overline{d F(c_1 - x_1)}}{dc_1}.$$

If we suppose this expression formed for each of the points present in the vessel, and all the resulting expressions added, we obtain the total force exerted by all the points, or the pressure which the wall suffers from them. But the sum of all those expressions is no other than $-\dfrac{d U}{dc_1}$; and hence the product $-\dfrac{d U}{dc_1} \delta c_1$ represents the external work performed in displacing the wall to the extent δc_1.

What we have said of one wall holds good also for the other

five ; and hence we obtain the following result :—*The expression*

$-\Sigma\dfrac{d\mathrm{U}}{d\mathrm{c}}\,\delta c$ *represents the external work performed in the change*

of volume of the vessel.   At the same time, however, it is supposed that the walls suffer the same pressure during displacement as while they are stationary—or, in other words, that the change of volume takes place *in a reversible manner.*

The six terms of the sum $\Sigma\dfrac{d\mathrm{U}}{dc}$ can be immediately reduced

to three, because the differential coefficients for each pair of opposite walls, $\dfrac{d\mathrm{U}}{dc}$ and $\dfrac{d\mathrm{U}}{dc'}$, are equal, and hence we can put

$$\frac{d\mathrm{U}}{dc}\,\delta c + \frac{d\mathrm{U}}{dc'}\,\delta c' = \frac{d\mathrm{U}}{dc}\,\delta(c+c').$$

Here the sum $c + c'$ is the distance from each other of the two boundary planes of the parallelepiped which are perpendicular to the coordinate-direction we are considering.   Applying this to the three directions of coordinates, we get three terms with the variations of the three sides of the parallelepiped as factors. Now, as our parallelepiped has the three sides a, b, b, which are determined by the equations (81), and as those equations contain, besides a and b, only V as a variable, the variations of the sides can be represented by expressions in which there occurs only the one variation $\delta\mathrm{V}$.   After inserting these expressions we can contract the three terms into one ; and if the factor of $\delta\mathrm{V}$ then obtained be denoted by $-p$, the equation

$$\Sigma\frac{d\mathrm{U}}{dc}\,\delta c = -p\delta\mathrm{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad (100)$$

results, p representing a quantity which may be supposed very approximately equal to the pressure prevailing in the gas.   By means of this equation the form of equations (96), (98), and (99) can be again simplified.

§ 16. The equations obtained in the last section can be transformed so that their agreement with those which express the second proposition of the mechanical theory of heat shall come out still more clearly.   Equation (96) can be written thus :—

$$\delta\mathrm{U} - \Sigma\frac{d\mathrm{U}}{dc}\,\delta c = \tfrac{2}{3}\mathrm{T}\delta\log\mathfrak{U}.$$

Here $\delta\mathrm{U}$ represents the increment of the ergal, and consequently the internal work, and $-\Sigma\dfrac{d\mathrm{U}}{dc}$ the external work.   Hence the left-hand member of the equation expresses the total work performed

when the system passes from the one state to the other. If δL denote this work, the equation will be

$$\delta L = \tfrac{2}{3} T \delta \log \mathfrak{U}. \quad . \quad . \quad . \quad . \quad (101)$$

If in equation (98) the last term of the right-hand member be transposed to the left-hand side, besides the previously discussed quantities of work, there also occurs here, contained in δE, the quantity δT, which represents the increment of the *vis viva*, and in the equations of the theory of heat signifies the increment of the heat present in the body. Now, as the increment of the heat present and the work done (for which also heat must have been expended) are together equal to the total heat communicated (which we will designate by δQ), we have

$$\delta Q = \tfrac{2}{3} T \delta \log \mathfrak{E}. \quad . \quad . \quad . \quad . \quad (102)$$

The above equations are in accord with those derived in my earlier memoir[*], except in one point, which is one of those in which mine agreed with those previously advanced by Boltzmann[†]. I have already alluded to this point : in correcting an article, in which I made clear the difference between Boltzmann's treatment of the subject and my own[‡], I added the following note :—" Since sending this article to the printer, I have found in pursuing my investigations that the expressions in question need, in order to be universally valid, a further alteration, which will make them still more different from Boltzmann's." It is through this change that my new equations differ from my previous ones.

The equation which I formerly advanced[§] for the determination of δL can, by some slight transformations, be brought into the following form,

$$\delta L = 2 T \delta \log \mathfrak{R},$$

where $\mathfrak{R}$ denotes a quantity very nearly proportional to the mean length of path of the molecules. Now, as we have seen, the mean length of path is nearly proportional to the volume of a given quantum of gas ; hence $\mathfrak{R}$ is also approximately proportional to the volume. Accordingly the variation $\delta \log \mathfrak{R}$ and the variation in (101), $\delta \log \mathfrak{U}$, may be regarded as answering to one another. The factors, however, of these variations do not correspond, the former variation having the factor 2T, while the latter has for its factor $\tfrac{2}{3}$T.

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[*] *Sitzungsberichte der Niederrhein. Ges. für Natur- und Heilkunde,* 1870; Pogg. *Ann.* vol. cxli.; Phil. Mag. S. 4. vol. xlii.
[†] *Sitzungsberichte der Wiener Akademie,* 1866, vol. liii.
[‡] Pogg. *Ann.* vol. cxliv. p. 265.
[§] *Sitzungsberichte der Niederrhein. Ges.* 1870; Pogg. *Ann.* vol. cxli. p. 458; Phil. Mag S. 4. vol. xlii. p. 179.

When, applying the ordinary laws of gases, the work is determined which is accomplished in a change of state, it is found that the factor $\frac{2}{3}T$ is the correct one, and consequently that the earlier equation, while in every thing else its form is correct, does not in this point correspond to the reality.

I must confess that this divergence of the then existing formulæ from the reality (to which my attention was first turned subsequently) has occasioned me great difficulties. Through it I was obliged to begin the entire mechanical treatment of the subject *de novo*, and to carry it out in a more generalized manner. In doing so I convinced myself that my earlier equations were certainly sufficient for motions of points in closed paths, but that new considerations, not yet instituted in mechanics, were requisite for that extension of the equations which would make them suitable to be applied to a system of points that do not move in closed paths. By this investigation I arrived at the theorem of the mean ergal, which, because it appeared to be of general importance in mechanics, I published in a separate memoir, and which in the present one I have applied to the molecular motions of gases; at least I have replaced these motions by others which have the same mean ergal and the same mean *vis viva*, and with which the variables and time-intervals which satisfy the conditions of the theorem can be readily obtained. I think that this result is peculiarly adapted to show how necessary it is to contemplate from this new point of view the second proposition of the mechanical theory of heat, if we wish to reduce it to mechanical principles.

§ 17. The molecular motions of gases are frequently considered in this way :—The molecules are represented simply as elastic balls which exert no forces on one another except after their surfaces have come into contact, when on a still nearer approach they repel one another with a rapidly increasing force. This representation is defective, even for the case in which, neglecting the relative motions of the constituents of a molecule, we wish to take into consideration only the motions of the centres of gravity of the molecules, because, as we must conclude from certain phenomena, the molecules not merely repel, but at somewhat greater distances also attract each other. Still the conception may serve to give an idea of the nature of the motion and to explain some characteristic properties of gases. It will therefore not be devoid of interest if we develop a little more completely the equation for the case *where there exists between the molecules only a repellent force which first becomes sensible when they arrive at a certain degree of proximity, but then, as they approach still closer, increases very rapidly.*

First replacing, as before, the actual motions of the molecules

within a certain volume V by motions of material points in a rectangularly parallelepipedal vessel, and employing coordinates parallel to its sides and having their origin in its centre, we express the forces exerted on a point by the wall that is perpendicular to the x-axis, not generally by $F'(c-x)$ and $-F'(c+x)$, but, as in § 5, by

$$-m\frac{n\alpha^n}{(c-x)^{n+1}} \quad \text{and} \quad m\frac{n\alpha^n}{(c+x)^{n+1}}.$$

We can then make use of all the formulæ developed in §§ 6, 7, and 8, and can, indeed, further simplify them, by now assuming that the quantity α (which we there assume to be so small that the force in the centre of the vessel was insensible) is so small that the force is sensible only in the immediate vicinity of the side—and therefore, in developing the series, taking into account only the terms of the first order with respect to α.

Of the equations given in those sections we will select equations (53) and (54) for further discussion, but will abbreviate them by omitting the terms which contain as a factor a higher than the first power of β, which is of the same order as α. In this form they read :—

$$h = \frac{2}{n}\frac{\beta}{c}w^{\frac{n-1}{n}}$$

$$\log u = \log(16mc^2) - 2\frac{n-1}{n}\frac{\beta}{c}w^{-\frac{1}{n}}.$$

We have previously advanced these equations for a single movable material point only ; we will now derive from them equations valid for the entire system of points to be considered.

For this purpose, in the former we substitute for w the product $\mathfrak{w}z^2$ and multiply it by $f(z)dz$, thereby getting

$$hf(z)\,dz = \frac{2}{n}\frac{\beta}{c}(\mathfrak{w}z^2)^{\frac{n-1}{n}}f(z)\,dz.$$

Integrating this equation from $z=0$ to $z=\infty$, we obtain on the left-hand side the arithmetic mean of all the occurring values of h, which, as before, we will denote by $\bar{h}$; this gives

$$\bar{h} = \frac{2}{n}\frac{\beta}{c}\mathfrak{w}^{\frac{n-1}{n}}\int_0^\infty z^2{}^{\frac{n-1}{n}}f(z)\,dz. \quad . \quad . \quad (103)$$

We make the same substitution in the second of the above equations ; but we multiply it by $z^2f(z)dz$, from which results :—

$$\log u \cdot z^2f(z)dz = \log(16mc^2) \cdot z^2f(z)dz$$
$$- 2\frac{n-1}{n}\frac{\beta}{c}(\mathfrak{w}z^2)^{-\frac{1}{n}}z^2f(z)dz.$$

Integrating this from $z=0$ to $z=\infty$, we get, on the left-hand side, according to (91), $\log \mathfrak{u}$; and on the right-hand side, according to (84) we have to suppose

$$\int_0^\infty z^2 f(z)\,dz = 1,$$

so that the equation changes into

$$\log \mathfrak{u} = \log'(16mc^2) - 2\frac{n-1}{n}\frac{\beta}{c}\mathfrak{w}^{-\frac{1}{n}}\int_0^\infty z^{2\frac{n-1}{n}}f(z)\,dz. \quad (104)$$

In order to calculate the integral which occurs in equations (103) and (104), a special assumption would have to be made concerning the function f, which determines the ratio of the velocities of the different points. Taking Maxwell's law as basis, according to which the expression $\sqrt{\dfrac{2}{\pi}}e^{-\frac{1}{2}z^2}$, given in (85), would have to be substituted for $f(z)$, we should obtain

$$\int_0^\infty z^{2\frac{n-1}{n}}f(z)\,dz = \sqrt{\frac{2}{\pi}}\int_0^\infty z^{2\frac{n-1}{n}}e^{-\frac{1}{2}z^2}\,dz,$$

by which we should arrive at a gamma function, namely

$$\int_0^\infty z^{2\frac{n-1}{n}}f(z)\,dz = \frac{1}{\sqrt{\pi}}2^{\frac{n-1}{n}}\,\Gamma\frac{3n-2}{2n}. \quad . \quad (105)$$

For the following developments, however, it is not necessary to know the value of that integral; we may be content to use an abbreviated symbol for it; and for this purpose we will put

$$Z = \int_0^\infty z^{2\frac{n-1}{n}}f(z)\,dz. \quad . \quad . \quad . \quad . \quad . \quad (106)$$

The two equations then become:—

$$\left.\begin{aligned}
\bar{h} &= \frac{2}{n}Z\frac{\beta}{c}\mathfrak{w}^{\frac{n-1}{n}}; \\
\log \mathfrak{u} &= \log(16mc^2) - 2\frac{n-1}{n}Z\frac{\beta}{c}\mathfrak{w}^{-\frac{1}{n}}.
\end{aligned}\right\} \quad . \quad . \quad (107)$$

We will now apply these equations to the three directions of the coordinates singly; and form the sum of the three equations which thereby arise out of each of them, thus:—

$$\left.\begin{aligned}
\bar{h}_1 + \bar{h}_2 + \bar{h}_3 &= \frac{2}{n}Z\beta\left(\frac{1}{c_1} + \frac{1}{c_2} + \frac{1}{c_3}\right)\mathfrak{w}^{\frac{n-1}{n}}; \\
\log(\mathfrak{u}_1\mathfrak{u}_2\mathfrak{u}_3) &= \log\left[(16m)^3c_1^2c_2^2c_3^2\right] - 2\frac{n-1}{n}Z\beta\left(\frac{1}{c_1} + \frac{1}{c_2} + \frac{1}{c_3}\right)\mathfrak{w}^{-\frac{1}{n}}.
\end{aligned}\right\}(108)$$

We multiply the first of these equations by $\frac{1}{2}Nm$. The left

side then represents the total ergal U, and we get

$$U = Nm \frac{Z}{n} \beta \left(\frac{1}{c_1} + \frac{1}{c_2} + \frac{1}{c_3} \right) \mathfrak{w}^{\frac{n-1}{n}}. \quad . \quad . \quad . \quad (109)$$

We multiply the second equation by $\frac{1}{2}$, and then subtract $\frac{1}{2} \log (4m)^3$ from both sides. The left side then represents, according to (95), the quantity $\log \mathfrak{U}$, and consequently there comes

$$\log \mathfrak{U} = \log (2^3 c_1 c_2 c_3) - \frac{n-1}{n} Z \beta \left(\frac{1}{c_1} + \frac{1}{c_2} + \frac{1}{c_3} \right) \mathfrak{w}^{-\frac{1}{n}}. \quad (110)$$

In referring these equations to the particular parallelepiped with the sides a, b, b, we have to substitute $\frac{1}{2}a$, $\frac{1}{2}b$, $\frac{1}{2}b$ for the quantities c_1, c_2, c_3, and then to bring into use equations (81), whereby we obtain

$$2^3 c_1 c_2 c_3 = ab^2 = V - \epsilon,$$
$$\frac{1}{c_1} + \frac{1}{c_2} + \frac{1}{c_3} = \frac{2}{a} + \frac{4}{b} = \frac{S}{V - \epsilon};$$

so that the preceding equations will be :—

$$\left. \begin{aligned} U &= Nm \frac{Z}{n} \beta \frac{S}{V - \epsilon} \mathfrak{w}^{\frac{n-1}{n}} ; \\ \log \mathfrak{U} &= \log (V - \epsilon) - \frac{n-1}{n} Z \beta \frac{S}{V - \epsilon} \mathfrak{w}^{-\frac{1}{n}}. \end{aligned} \right\} \quad . \quad . \quad (111)$$

If we denote, as before, the total *vis viva* of the system by T, then

$$T = \frac{1}{2} N 3m \mathfrak{w}, \text{ and hence } \mathfrak{w} = \frac{2}{3} \frac{T}{Nm},$$

by which the equations are changed into :—

$$\left. \begin{aligned} U &= \left(\frac{2}{3} \right)^{\frac{n-1}{n}} \frac{Z}{n} \beta \frac{S}{V - \epsilon} T \left(\frac{T}{Nm} \right)^{-\frac{1}{n}} ; \\ \log \mathfrak{U} &= \log (V - \epsilon) - \left(\frac{3}{2} \right)^{\frac{1}{n}} \cdot \frac{n-1}{n} Z \beta \frac{S}{V - \epsilon} \left(\frac{T}{Nm} \right)^{-\frac{1}{n}}. \end{aligned} \right\} (112)$$

If by V we understand specially *that volume which contains a unit weight of the gas,* so that the masses of the molecules contained in it together form a unit of mass, we have to put $Nm = 1$. For this case we will, for simplification, introduce the symbol γ, the meaning of which is determined by the equation

$$\gamma = \left(\frac{2}{3} \right)^{\frac{n-1}{n}} \frac{Z}{n} S \beta, \quad . \quad . \quad . \quad . \quad (113)$$

or also, according to (29), by

$$\gamma = \left(\frac{2}{3}\right)^{\frac{n-1}{n}} \frac{Z}{n} SR\alpha, \quad \cdots \quad (113a)$$

from which it is evident that γ is a small constant quantity. The equations will then read :—

$$\left.\begin{array}{l} U = \dfrac{\gamma}{V-\epsilon} T^{\frac{n-1}{n}}; \\[2mm] \log \mathfrak{U} = \log (V-\epsilon) - \dfrac{3}{2}(n-1)\dfrac{\gamma}{V-\epsilon} T^{-\frac{1}{n}}. \end{array}\right\} \quad (114)$$

This equation for U gives forthwith also those for E and U−T; and in like manner, taking into consideration (97), in which Nm is also to be put $=1$, we can derive from the equation for $\log \mathfrak{U}$ those for $\log \mathfrak{E}$ and $\log \mathfrak{J}$, viz.

$$\left.\begin{array}{l} E = T\left(1 + \dfrac{\gamma}{V-\epsilon} T^{-\frac{1}{n}}\right), \\[2mm] \log \mathfrak{E} = \log\left[(V-\epsilon T)^{\frac{3}{2}}\right] - \dfrac{3}{2}(n-1)\dfrac{\gamma}{V-\epsilon} T^{-\frac{1}{n}}, \end{array}\right\} \quad (115)$$

$$\left.\begin{array}{l} U - T = -T\left(1 - \dfrac{\gamma}{V-\epsilon} T^{-\frac{1}{n}}\right), \\[2mm] \log \mathfrak{J} = \log \dfrac{V-\epsilon}{T^{\frac{3}{2}}} - \dfrac{3}{2}(n-1)\dfrac{\gamma}{V-\epsilon} T^{-\frac{1}{n}}. \end{array}\right\} \quad (116)$$

Consequently the six quantities to be determined are represented in a simple manner as functions of T and V; and it is easy to convince one's self that these functions give the mutually accordant differential coefficients

$$\frac{d_v U}{d \log \mathfrak{U}} = \frac{d_v E}{d \log \mathfrak{E}} = \frac{d_v (U-T)}{d \log \mathfrak{J}} = \frac{2}{3} T.$$

At the same time it may be again mentioned that the equations of this last section are only to be applied to the molecular motions of gases when we neglect the attraction of the molecules —which of course alters the value of the ergal, and may even affect its sign.

XXIV. *On the Origin and Mechanism of production of the Prismatic (or columnar) Structure of Basalt. By* ROBERT MALLET, *F.R.S.*

[Concluded from p. 135.]

WE have now to consider what will be the form of the transverse fractures producing these joints. Referring to fig. 6 (being an axial section of the topmost portion of a prism down to the first joint *l m*, the axial surface of section in the

FIG. 6
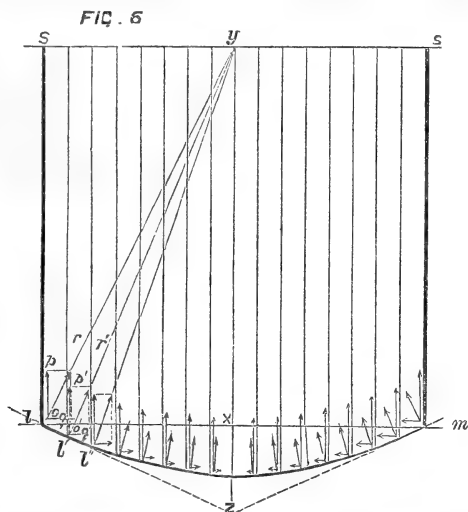
figure being divided by nearly parallel lines, the distances between which represent successive approximately isothermal *couches*), the prism is at once in progress of cooling and of contracting by loss of heat from its top surface S S, and by that from the sides of the prisms—which are at the lowest temperature at the top (where cooling has been going on longest), and higher at every point below down to the splitting level B B, fig. 4. In a line round the prism, as *l m*, fig. 6, every particle of the exterior *couche* is subjected to two orthogonal strains, due to contraction of the material of the prism in its axial and transverse directions respectively; and the contractile pull in each of these directions is proportional to the length of the contracting column which produces it. Thus at the point *o* (taken very near to *l*) the vertical strain *o p* is proportional to *l s*, and the horizontal or transverse component *o q* is proportional to *l x*, or nearly to the radius of the prism. The resultant, *o r*, of these two components is the direction of the contracting force at the point *o* tending to initiate fracture at that point; and if we assume

similar points taken all round the prism (which, as we shall see hereafter, it will be sufficiently exact to consider as a cylinder), the locus of the resultant through all these points will be in a cone $l\,y$, fig. 6.   In a homogeneous body the plane of a fracture when produced by contractile forces is perpendicular at every point to these forces.   The first portion of the fracture through the exterior *couche* through any point as, *o*, will not be directly transverse to the axis of the prism, but in a plane normal at that point to *o r*; and taking successive points round the entire circumference, the plane of fracture between *l* and *l′* will approximate to a cone whose vertical angle will be *l z m*.

Proceeding now to the next isothermal *couche* within, we may apply the same reasoning to find the direction in which the fracture will proceed through it.   If *o′* be the point at which the fracture through the outside *couche* is prolonged into the second, then *o′ p′* will be the vertical component and very nearly the same as in the former case ; but the horizontal component *o′ q′* is less than for the outside *couche* by the assumed thickness of that couche, the resultant or rending pull *o′ r′* is therefore less inclined for this, the second, than for the previous or outside *couche*, and the fracture through the thickness of the second *couche* normal to *o′ r′* is more nearly transverse to the axis of the prism than that of the outside *couche*; and so on for every successive *couche* until we arrive at the axis of the prism, where the plane of the fracture will be exactly transverse to it.   Assuming the successive *couches* of indefinitely small thickness, the entire fracture through the prism will be thus in the form of a lens at the convex side, or of a corresponding shallow cup at the concave side— the convex side of the curve of fracture always presenting itself in the direction opposite to that in which the cooling of the prism is occurring, or towards the hotter part of the prism.

Referring now to fig. 7 (which represents an axial section below the first cross joint, already referred to, and between that and the second joint), it will be obvious that the curvature of the cup-shaped fracture will slightly differ from that of the first joint as in fig. 6.   For at the point *o*, corresponding to that in fig. 6, the vertical component *o p* is the same as in fig. 6, and so is *o q*, and the angle of the resultant *o r* is also the same ; therefore the angle of the plane of fracture at the surface of the prism is the same as in fig. 6 ; but as the top surface of the whole mass of basalt, assumed as a horizontal plane S S in fig. 6 is the origin of all the vertical components down to the first joint, so these increase slightly in succession as they approach the axis : but after the first fracture has been effected, the origin of the vertical components producing the second and all subsequent joints is to be found in the hollow surface of the pre-

ceding joint above it; the vertical components of fracture for every joint below the first one are therefore equal, and the hori-

FIG. 7
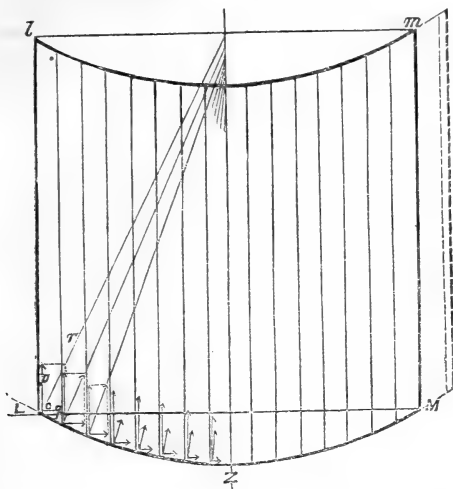
zontal components alone vary, as already described, from *l* to the axis. The effect of this is simply that the amount of curvature of every joint below the first is rather greater than that of the first, or, in other words, the depth of the cup-shaped articulation of the first joint produced is a little less than that of all subsequent joints proceeding lower down.

We have thus proved that contraction alone in the axial and transverse directions is sufficient to account for the production of transverse joints, and their cup-shaped form of fracture taking place at successive intervals as the cooling proceeds along or downwards through the prism, and that, if the material be isotropic and homogeneous, the distance between any two successive joints must be approximately equal for like conditions of cooling; and we can see that the amount of curvature of each joint, and the nature of any curve in any plane passing through the axis of the prism, depend upon the relation that subsists between the length of the piece between two successive joints and the diameter of the prism, and the law according to which heat is lost in directions transverse to the axis of the prism. It would be difficult, if not impossible, to assign precisely the positions and directions of the isothermal *couches*, as shown in fig. 4 and subsequent figures, in so complicated a case of cooling as that which is here before us; but it will be readily seen that neither the form nor the amount of concavity in the joints will

be very materially varied, whatever be the law of lateral cooling which we may be at liberty to assume. In figs. 6 and 7 the thickness of successive isothermal *couches* has been taken as equal, or the volume of a unit length of each *couche* to vary as the square of the radius, thus giving the largest volume to the outside and hottest *couche*. But if the thickness of the isothermal *couches* be taken so as to give equal volumes per unit length to all the successive couches from surface to centre, the curvature of the cross fracture L Z M in fig. 7 is but little altered by the difference in the rate of cooling inferred, and approaches more nearly to an elastic curve than that shown by fig. 7, which is not far from that of a circle. If the thickness of the exterior *couches* be supposed still less than in either of those cases, the effect upon the curvature will be to cause it to approach more nearly to that of an ellipse, so that the cup-shaped articulations would approach more nearly to the form of a watch-glass.

In the preceding considerations we have viewed as a sufficiently near approximation that the hexagonal prism coincides with the inscribed cylinder. But those portions of the prism exterior to that cylinder have also to be considered; and, as will be seen by the dotted lines to the right of fig. 7 (representing in so far an axial section of the prism taken through the angles of the hexagon, in which the curve of transverse fracture has been laid down on the principles already described), they present no material difference as to the curvature where that approaches the solid angles of the prism, from the same curve as laid down in fig. 7 taken in a direction of section normal to the faces.

In all that precedes we have supposed the cooling of the basaltic tabular mass to take place from the top surface only, when the convexity of the cross fractures will point downwards only. But the cooling of such a tabular mass may take place either from the top or bottom surface, or from both, in which cases the convexity of the cross joints will point upwards at the lower portions of the mass; and *in general the convexity of the cross joints will point in the reverse direction to that in which the wave of heat has been transmitted from any cooling surface of the mass.* It is obvious that the curvature of any cross joint must be less in proportion as the diameter of the prism is less—that is to say, as the difference in temperature at the time of fracture is less between the exterior and axial portions of the prism. Hence in very small prisms we should expect to find the transverse fractures more nearly approaching to a direct transverse plane of separation, as is actually seen to occur in the long and slender prisms of the Rhineland basaltic country: but this will be better referred to further on in considering the effect of want of homogeneity in the mass upon the form and positions of the vertical and cross jointings.

It remains here to explain the play of forces by which the lip
or edge of the cup adjacent to the solid angles of the prism is
so frequently found broken off, or strained so as readily to break
off, in something of the peculiar form shown in fig. 8 in vertical
section, and in the plane of the joint in fig. 9.   It will be re-

FIG. 9

FIG. 8

SECTION THROUGH A.B.
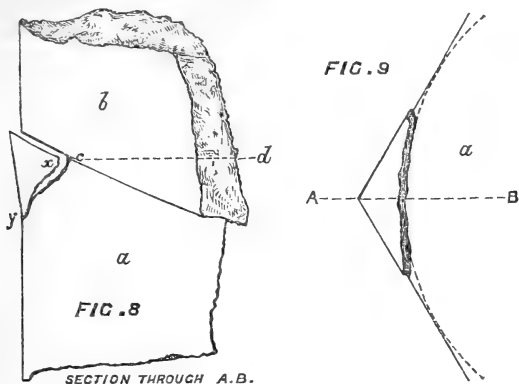
collected that at the moment of the production of the cup-joint
the material at the concave side *a* is still at a somewhat higher
temperature than the mass *b* from which it has just become de-
tached by a fracture, the sides of which are kept in absolute con-
tact by the superincumbent weight of *b* and those above it.

Cooling still proceeds, and with it contraction until both *b*
and *a* have attained the same temperature; but the form of the
cup-shaped fracture has been impressed upon it at a higher tem-
perature as respects the lower block *a*, which therefore differen-
tially contracts as to its cup-shaped cavity upon the lens-shaped
surface of *b* which fills it; hence strains transverse to the prism
and in the plane of the joint are produced, the effect of which is to
tend to split off portions of the exterior parts of the cup *a*; and
this action is most effective at and about the solid angles of the
hexagon, where the surfaces of the cup-shaped fracture have the
most effective frictional grip of each other, *i. e.* where the angle
of the fracture itself is largest with respect to a plane passing
through or near the lip of the cup, as *c d*, fig. 8.   This play of
forces may either break off altogether three or the whole of the
solid angles of the hexagon at the lip of the cup, or may only
visit so severe a strain upon the rigid basalt at and about these
parts as may cause a plane of weakness by which, at some sub-
sequent period, by any slight extraneous force such as frost &c.,
the fracturing off of these fragments may be produced, as seen
in figs. 8 and 9.   The curves assumed by these fractures are
always such as indicate the play of forces as above described,

ending in a thrust outwards upon the detached fragment, the fracture of which may be longer or shorter between x and y, dependent upon the rigidity of the basalt and its coefficient of contraction, which no doubt varies a good deal in different examples of that rock.

From what precedes, it is obvious that in a perfectly homogeneous and isotropic mass of basalt cooling symmetrically, all the prisms should be divided transversely by joints, each range of which should be at the same level in all the prisms—an arrangement which observation shows is very nearly approached in many instances in ponderous masses of basalt; but a very slight amount of heterogeneity or admixture of adventitious matter, or difference in the rate of cooling, will cause a certain amount of irregularity in the relative levels of the different prisms or length of the joint pieces. It is obvious also that the imbedding of foreign material, or even the existence of empty cavities, which are often of considerable size in certain basalts, will not only produce irregularities in the distances between these joints, but also more or less disturb the regularity of their cup-shape. This last source of disturbance in the cup-forms of the joints has been pointed out by Professor James Thomson in his paper already referred to. On referring to those papers it will be remarked how widely his notions of the production of the cross joints themselves differ from the solution here given, and how entirely inadequate they are to account for the facts. He supposes cross fracture to commence at the axis of the prism and to spread outwards towards its surface. He assumes tension, therefore, greatest about the axis or hottest part of the prism; and to account for the equal and opposite force, he imagines a *couche* produced about the exterior of the prism by calling in some wholly imaginary force of expansion produced by some molecular or chemical change in the basalt itself at and near the surface of the prism by which its length there is increased. Even were we to admit for a moment these vague and unsupported hypotheses, there is nothing indicated to account for the production of the cup-form of the joints, nor for their convex sides being presented in one direction rather than another, nor why they should actually be presented in opposite directions.

Hitherto we have been considering perfectly homogeneous and isotropic basalt; and although such basalt is closely approached in nature, observation shows that almost all basalts are more or less heterogeneous as regards the mineral constituents of which they are made up or which are imbedded in them, and to a greater or less extent are not isotropic anterior to cleavage into prisms. The main constituent minerals, namely felspar, pyroxene, oxides of iron, &c., vary much in proportion and in the

development of their differently shaped crystals. The entire mass, forced up through fissures from below and overflowing horizontally, assumes the interior conditions of a stream or streams of imperfectly liquid matter, in which the floating crystals arrange themselves with their broader sides more or less horizontally, and their greater lengths in the direction of the flow. A distinct grain is thus often impressed upon the entire mass, tending to produce differences in the facility of fracture in two vertical orthogonal planes and one horizontal plane, the latter being the one of easiest fracture in the mass. From the fineness of the particles of basalt and the want of large imbedded crystals, these characteristics are but ill pronounced and are frequently not discernible; but they only differ in degree from the well-known structure, likewise developed by flow, which has been remarked, and is well known to the quarry-men in the Dartmoor and many other granites, which possess three orthogonal planes presenting differences of facility of fracture. The horizontal one is that of easiest fracture; and in it are found the flat surfaces (generally) of the large separate crystals of felspar, and many of those of black mica also. The plane of next easiest fracture is a vertical one, which generally coincides with the longest axes of these crystals; while the plane of most difficult fracture is a vertical one orthogonal to the last, and therefore transverse to the line of flow of the granite when molten. In addition to this structure, more or less developed, the lower part, which has received the greatest pressure from the superincumbent mass of the basalt itself and of matter heaped upon it, must be denser than the higher portions. All these conditions tend to modify several features of the prismatic structure, as already developed in what precedes. Different parts also of the same large mass of basalt will be found on comparing hand specimens to differ in grain, hardness, &c., showing that the basalt when poured forth in a liquid state was not perfectly homogeneous throughout.

These differences involve variations of elasticity and rigidity in different directions, and, we cannot doubt, variations in extensibility under contractile strains, and in the rate of cooling under given conditions. The principal effects produced by all these are chiefly shown in variations of the mean diameter of the prisms and in the length between the cross joints. Where the lengths between these are usually found to be very great, as in most of the Rhenish basalts, the prisms are found generally of small diameter, often not exceeding six or seven inches; and in such cases the nearly flat cross joints, often many feet apart, are attributable not so much to the differential contraction in cooling, already described as generating cup-formed joints, but chiefly to the propagation of mechanical strains through the whole mass,

breaking the prisms transversely, or pulling asunder lengthwise or breaking transversely such prisms as may be unequally gripped at different points by those surrounding them—these strains originating in the later stages of cooling of the mass viewed as a whole.   On the other hand, where planes of easiest fracture are more or less developed in the mass of unsplit-up basalt, their effect will be to shorten the length between the joints, so that they shall be found to occur within distances of each other less than the diameter of the prism.   This is frequently observed in the Antrim and Scottish basalts; and wherever that is the case, so far as the writer has observed, the separate-joint blocks break under the hammer more easily in the direction transverse to the length of the prism than in any other.   We have yet to consider some of the circumstances which render the prisms into which the mass splits more or less irregular as to the lengths and the number of their sides.   As has been shown, in homogeneous and isotropic basalt cooling symmetrically, the prisms can only be hexagonal and equilateral.   Irregular prisms, however, are to be found in most basaltic regions, having from three to seven or even more sides, and often far from equilateral.   Without going into prolix detail, it will be easily seen that differences of molecular structure in different directions in the yet unsplit mass, the unequal dispersion throughout of cavities or heterogeneous imbedded materials, differences of conductivity in nearly contiguous parts of the mass, slight differences in the contraction in different directions and the consequent greater width of the fissures separating the prisms in one direction rather than in another, and slight differences in the rate of cooling of adjacent parts of the mass by inequalities in the contour of the cooling surface or in the "jacketing" of detrital matter which may cover it, or in the movements of superincumbent water, all tend respectively to perturb the regularity of the hexagonal prism, which, however, is never observed to disappear as the ruling or typical form of the basaltic prism.   Walking over the tops of a large pavement of basaltic columns such as those exposed at the Giant's Causeway and elsewhere, one sees places in which the hexagonal form becomes irregular, and gives place, though for a very small extent of surface, to quadrangular and other irregular forms, sometimes even triangular or wedge-shaped; but passing a short distance we find these rapidly returning to the normal hexagonal type through intervening prismatic forms, which usually plainly indicate that, whatever may have been the originating cause of the disturbance of form, the tendency is to return to the hexagonal prism through such intermediate forms of irregular prisms as require the least amount of molecular work to produce their separation, the principle of least action thus being ever and pervadingly present.

So far we have been regarding the mass of basalt as tabular and horizontal, and cooling symmetrically from the top or bottom surface or from both, in which cases the prisms are all vertical and straight. We have yet to point out how differences in the form, variations in thickness at different parts, and in the positions of the principal cooling surfaces, can affect the position in space of the axes of the prisms, and can cause them to be no longer straight but curved in very various ways. On the principles already developed, it is plain that the direction of the axes of the prisms is determined by the form of the surface passing through all the points in which splitting is actually taking place, the separated sides of the prisms being always normal to that surface. If, therefore, the mass be tabular and horizontal, and cooled from the top surface only and uniformly from it, the prisms will be vertical and straight, and will extend from the top to the bottom of the mass, where they will rest upon a thicker or thinner stratum of irregular fragments, as in fig. 10. If, however, a like tabular mass be cooled from the top surface and the bottom surface also, there will be formed two distinct ranges of straight vertical prisms abutting upon each other somewhere between the top and the bottom of the mass, and separated by a stratum of irregular angular fragments as in fig. 11 : the upper range

FIG. 10
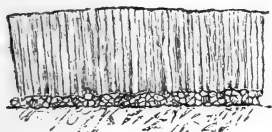
FIG. 11
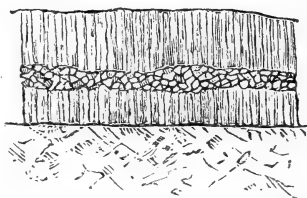
of prisms is produced by the top cooling causing splitting downwards, the lower range by the cooling from the bottom and splitting upwards. The axes of any two prisms in the top and bottom ranges need not coincide in position; and the respective lengths of the prisms in each range depends upon the rate of cooling from the top and from the bottom respectively; so that if the cooling from each of these were the same, the separating stratum of irregular fragments would be found in the middle of the thickness of the mass. But in nature it almost always occurs, for obvious reasons, that the rate of cooling from the top surface greatly exceeds that from the bottom, and hence the upper range of prisms is proportionately longer than the lower range. Where the thickness of the mass is nearly level on the top and is moulded to slight irregularities of the ground over which it has

been poured, and where the mass, whether from being relatively thin, or from having been poured out over a surface of badly conducting material, is mainly cooled from the top surface only, then the prisms may be all vertical and straight throughout the mass, the stratum of broken fragments being at the bottom but varying in thickness, that being greatest about the summits of the inequalities of the bottom.   But where (as in fig. 12) the entire thickness of the mass is great, the top surface being, as before, nearly level, but the bottom surface presenting considerable inequalities, and cooling taking place from the top and also from the bottom, then the upper range of prisms will still be vertical, straight, and parallel to each other; but the lower range of prisms formed will be alternately divergent and convergent and present more or less irregularities, and be separated from the upper range by a curved stratum of irregular fragments, the general contour of which will have a certain resemblance to the curvilinear forms of the bottom upon which the mass was poured out.   If the basalt be poured forth beneath, or prior to its cooling be covered over by a thick stratum of detrital or other badly conducting material, so that the cooling takes place almost wholly from the largely irregular bottom surface (as in fig. 12 *bis*), then

FIG. 12
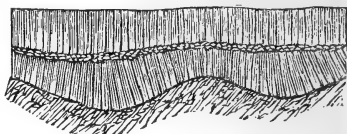
FIG. 12 *bis*
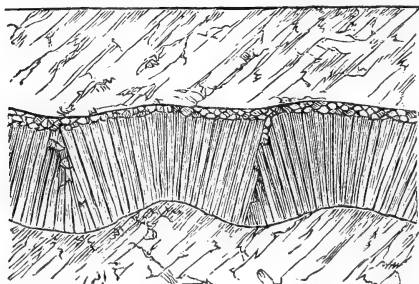
will the prisms be alternately convergent and divergent throughout the entire thickness of the basalt, their uppermost portions being separated from the detrital covering by a layer, variable in thickness, of irregular fragments of basalt   Should the molten mass have been forced up through a wide vertical or more or less inclined fissure of large size, which it fills to the surface but does not overflow (as in fig. 13), then cooling from both sides of the fissure will produce two ranges of prisms straight and perpendicular

to the walls of the fissure, and separated by a stratum of irregular fragments, which will be in the middle of the dyke if both walls of the fissure before its filling were of equal conductivity and at the same temperature. The upper part of the dyke will consist of a confused assemblage of partly prismatic, partly irregular fragments, due to the irregularity of the cooling there, both from the sides and the top*. If the mass have its top surface level (but, as in all the cases refered to, covered with more or less irregularities which do not affect the main result) and the mass abut against a vertical or nearly vertical surface of rock or other solid matter (as in fig. 14), there will be two ranges of prisms formed—

FIG. 13

the one by cooling from the top, straight and vertical, the other by cooling from the abutting side, straight and perpendicular to that side; and these two ranges, where they would intersect each other, will usually be separated by a thinner or thicker stratum of irregular fragments, inclined both to the top and to the abutting surfaces, and dividing the entire angle between these in a ratio which depends on the rate of cooling from the top and from the

FIG. 14

abutting side respectively, the prisms being longest in that direction in which cooling has proceeded most rapidly—the irregular fragments (in this as in all other instances in which they are referred to) being produced by irregular cooling at the extremities of the prisms, where the wave of heat passes no longer in a

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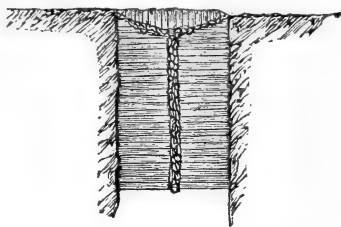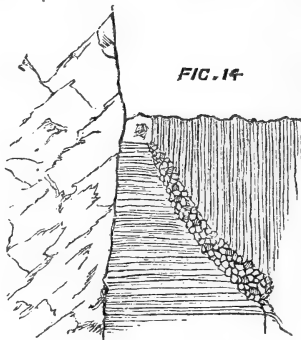
* These conditions appear to militate seriously against the views proposed by several authors as to the nature of those bright bands, often radiating from one point, which are seen telescopically upon various parts of the moon's surface—namely that these "Rillen" are dykes which have been filled by injection from below, but the molten matter filling them has not overflowed the lips of the fissures. Whatever might be the nature of the molten matter filling these fissures, it is scarcely conceivable that the broken-up top surface of the filling material should, after its consolidation, reflect more light than the solid material of the sides of the fissures, if there be any identity between the materials generally composing the moon's surface and those forming that of our earth.

single sheet, but is broken up into partial waves with divergent and, as may be said, accidental wave-paths, so that fracture by contraction takes place in many different directions. The strata of irregular fragments thus produced are not invariably found in nature, being sometimes replaced by thin beds of clays or ochres whose incoherence and small conductivity admit of more regular cooling at the extremity of the prisms. Such irregular fragments are also found occasionally converted into ochre by decomposition. If the thickness of a tabular mass varies but slightly and gradually, so that the section is slightly wedge-shaped, and cooling takes place mainly from the top surface, then the prisms may be straight and perpendicular to the top surface throughout the mass, and only separated from the bottom by a stratum of irregular fragments. But if (as in fig. 15) the bottom of the mass should be more highly inclined and cooling take place both from

the top and the bottom, then will there be two ranges of straight prisms formed, perpendicular respectively to the top and to the bottom surfaces, and separated by an intermediate stratum of irregular fragments—the respective

FIG. 15

lengths of the two ranges at any point depending on the relative rates of cooling, as already stated with reference to fig. 13 and others preceding it.
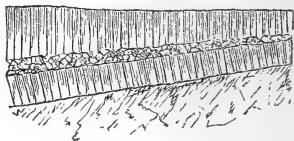
The directions of the axes of the prisms being dependent upon the contour passing through all the points of actual splitting at any instant (that is, upon the bounding contour of the original mass, as already fully explained), it is obvious that a mass presenting a curvilinear contour of cooling must produce divergent or convergent ranges of prisms, according as the cooling surfaces are convex or concave, the separating points of the prisms being every where normal to tangent planes passing through them at the surface of the mass (as in figs. 16, 17, and and 18)[*]. Convergent prisms must therefore be taper prisms; and it might be supposed that this tapering might continue until they all converge to a point, as do the crystals of certain minerals occurring in botryoidal or reniform and radi-

[*] The isolated aggregations of prisms leaning against each other and all sloping towards the centre (like hop-poles stacked in water), noticed by Mr. Scrope at several places in Auvergne, are the remaining or central portions of prismatic masses (as in the lower portions of figs. 18 & 19, and shown in Mr. Scrope's fig. 23, 'Volcanoes,' second edit. p. 94), all the other portions of the mass having been removed by denudation, as intimated by that author.

ated masses, such as arragonite, hematite, wavellite, &c. But observation proves that this is not the case; the taper prisms

FIG. 16

FIG. 17.
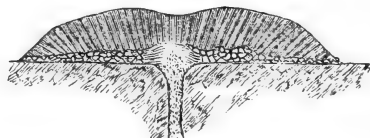
FIG. 18
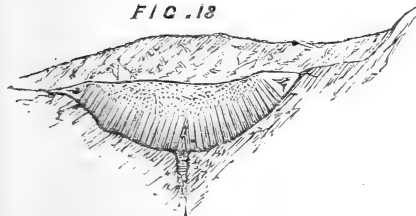
FIG. 19
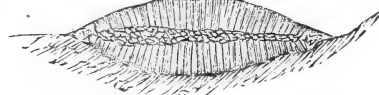
here, though in some respects simulating the forms of these taper crystals, are not produced by crystalline forces, but by the mechanical work of splitting up the cooling and contracting mass. The principle of economy in work, which determines the size as well as the form at starting, continues throughout the entire production of the prism however deep it may enter into the mass; but if each prism continued to diminish in diameter indefinitely in length, there would be an enormous waste of work in splitting the deeper parts of the mass into prisms far more numerous than would be necessary there to relieve the contractile strain, which, when the temperature has become the same is per unit length the same as it were at the surface. Hence after the taper prisms have reached a certain length from the surface, they are either found to end at a thin stratum of irregular fragments and a fresh range of taper prisms to commence (as in fig. 16 *bis*), whose diameters are the same as those of the prisms at the surface of the mass, or two or more of the taper prisms are fractured more or less transversely and irregularly, and, ending there, start off as a single prism of larger diameter and more or less irregular in form (as seen in fig. 16), all the prisms in the neighbourhood of such changes becoming more or less irregular, either as to form or number of sides or both. Examples of both these modes of adjustment are very frequent in Auvergne, the Cantal, and many other basaltic

FIG. 16 *bis*

regions, though no solution of the facts has hitherto, so far as the writer is aware, been given. In fig. 17 such convergent prisms are shown separating one of those huge mushroom-shaped protrusions of basalt which have welled up through fissures (as in fig. 13) and have also overflowed the surface, examples of which are numerous in Saxony and elsewhere. In such a case cooling is almost wholly confined to the top surface, and the convergent prisms with the interruptions already explained run to the bottom or near it, resting there on irregular fragments. Towards the edges of such masses (figs. 17, 18, 19) the basalt, by rapid cooling, may be not prismatic, but amorphous ; or if prismatic, the prisms may be more or less confusedly curved, on principles to be hereafter referred to. If (as in fig. 18) a hollow or valley be filled, whether by welling up from a fissure or supplied from some lateral flow which has introduced the mass of basalt beneath a thick covering of detrital and badly conducting material, then cooling may take place proceeding chiefly from the bottom, and so slowly that the contraction of the upper part of the mass may be sufficiently relieved by the subsidence of the upper portions *en masse*, in which case the columnar structure will be almost wholly developed by cooling from the under surface, and at a certain height above this columnar structure will become evanescent ; and all the remainder of the mass, except for a few inches or feet in depth, in contact with the detrital covering, where it will be broken up into irregular fragments, will be found to consist of massive basalt alone. Examples of this passage from prismatic into massive basalt are to be found in all basaltic regions.

Inasmuch as the splitting into straight prisms always takes place perpendicular to the cooling surface, it is obvious (on inspecting fig. 20) that in the event of basalt filling a valley of generally wedge-shape section, its top surface being covered by tufa or other badly conducting substance, so that the cooling of the mass takes place almost wholly by conduction to the sides of the valley, the prisms will be found in a sloping direction and generally perpendicular to both flanks of the valley respectively, being more or less irregular or confused as the valley-flanks themselves are so. If (as in fig. 21) the valley be broader and its

FIG . 20   FIG. 21
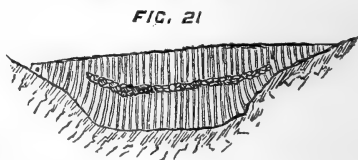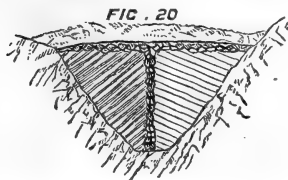
flanks more or less convex, and the mass of basalt be cooled

simultaneously from its top and bottom and from the convex flanks of the valley or hollow, the prisms will tend, on the whole, to perpendicularity towards the centre of the valley, being formed in one or several ranges separated by angular fragments; while at the convex flanks the prisms may assume curvilinear forms, on principles now about to be more particularly explained.

So far the writer has treated only of the formation of straight prisms; but curved or apparently bent prisms must also be produced by the play of those forces which have been already explained when the bounding surfaces supporting the mass of cooling basalt are curved, or the mass of such form as is suitable to their production.

Whatever be the form of a cooling mass of basalt whose temperature is supposed originally uniform throughout, the wave of heat which is first lost is parallel to the external contour of the mass; and if the mass be of such a form that successive isotherms are parallel to each other and to the original contour, as in the tabular masses that we have been considering, the prisms into which it splits, whatever be their direction in space must be straight and normal to the external contour or bounding surface of the mass. But if the mass be of such a form that successive isotherms taken in descending order (as ii, kk, ll, fig. 22) do not remain parallel, the prisms (as pp', $p'p''$, &c.) can be no
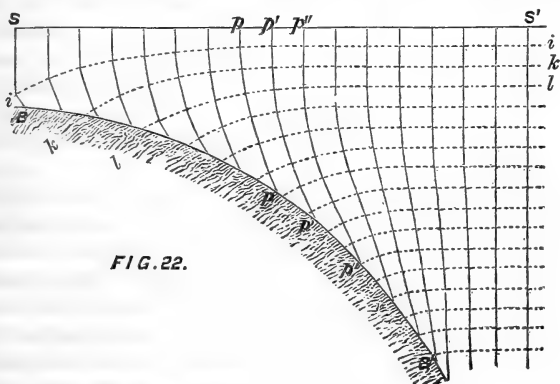
FIG. 22.

longer straight, because as successive isotherms at the splitting-temperature diverge from each other in one direction and converge towards the other, and as the splitting takes place normal to the isotherm or *couche* which is at the splitting-temperature, so the axis of each prism continually changes in direction as the splitting passes through successive divergent isotherms, as may be seen in figs. 22, 22 A, 22 B,—the first representing a mass with the top surface level, and the bottom a curve convex

upwards; the second a like mass, with the bottom concave;   and
the third a mass with the top surface convex and reposing upon

FIG. 22 A

FIG. 22 B
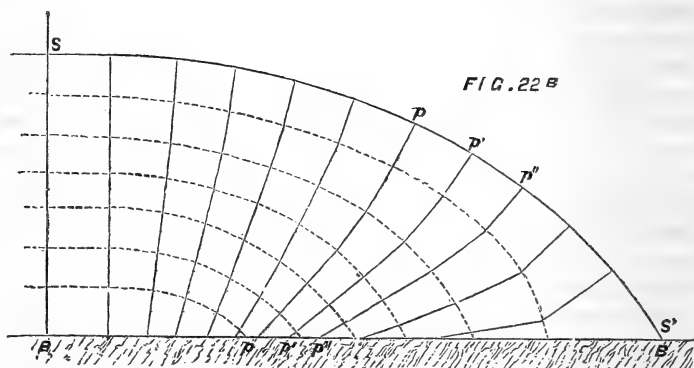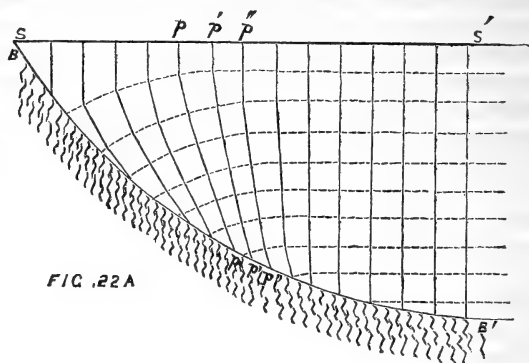
a level plane, cooling taking place in each case from the top sur-
face only.   The isothermal planes whose vertical sections are
represented in figs. 22, 22 A, 22 B by dotted lines will be seen to
diverge towards the shallower part of the mass in every case—just
as the ice which forms upon a pond, first forms and becomes
thickest at the shallower parts of the water, and thins out, it may
be to nothing, as it approaches the deeper parts—the under sur-
face of the ice (which at any instant is the isotherm of 32° Fahr.)
not being parallel to the level upper surface, but rising as it ap-
proaches the deeper and therefore still warmer portion of the
water, which, like our basalt, cools only from the surface.

The sole necessary condition, therefore, to the production of
curved in place of straight prisms is sufficient inequality in
volume, and therefore in the rate of cooling, of the mass at dif-
ferent points of its surface, the measurements being taken nor-
mally to the cooling surface; and curvature in the prisms must

be produced whether the form of the bottom upon which the mass of basalt reposes be convex (as in fig. 22), or concave (as in 22 A), or a plane (as in fig. 22 B).   Curvature in the prisms must also take place when the top and bottom surfaces are both planes (as in fig. 15) but inclined to each other at an angle large enough to give sufficiently abrupt change in depth to the cooling mass, cooling in all cases being supposed to take place from the top surface or from one of the two surfaces only.   The precise curve that will be assumed by each prism must vary in different parts of the mass, and may vary also in different parts of the length of each prism—that is to say, depends upon the greater or less rapidity with which the isotherms vary in divergence from parallelism.   The concave side of the curvature of each prism will always present itself towards the deeper part of the mass, in which direction also the isothermal planes converge.   The cases illustrated by the figs. 22, 22 A, 22 B refer to change of depth or of volume in the mass in one direction only, in which case the curvature of each prism will be in one plane only ; but, as is obvious, the depth or volume of the mass may vary in orthogonal directions, and hence the prisms split from such a mass may be curved in more than one plane ; and with certain forms of irregularity of the bottom the curvature of some of the prisms may have points of contrary flexure. It is further obvious that curved prisms thus produced by cooling from the top only will be very slightly taper, their smallest diameter being at the bottom.   As in nature the curvature is usually small in proportion to the length of the prism, so the taper may be so small as to be inappreciable.   It is also plain that if from any causes the rate of cooling varies at different points of the cooling surface, corresponding changes may result in the curvature or divergence of the isothermal planes and, by consequence, in the curvatures of the prisms themselves.

Innumerable variations and combinations of curvature, often so perplexed and confused as to defy analysis, are to be found in many basaltic regions, due generally to successive and superimposed outflows locally occurring, and to dykes filled with transverse prisms cutting through prismatic masses previously developed, and to violent diversities of cooling due to water-channels formed on the surface, or to violent mechanical efforts (such as those of tilting or dislocating of the bottom, or of denudation, &c.) having concurred with, or acted posteriorly to the production of the prisms themselves ; but into these details the writer cannot here enter.

No intelligible solution of the mode of production of curved basaltic prisms has, so far as the author is aware, been produced by former authors. De la Bèche (' Geological Observer,' 2nd edit.

p. 406) says, " also in those curved columns of basalt where . . . .
no joints are apparent we may suppose that some tendency of
an original set of spheroids to develop themselves more in one
direction than another, from some local cause, has been so con-
tinued as to produce the general curve observed ;" while Scrope
says (' Volcanos,' 2nd. edit. pp. 99–100), " sometimes the co-
lumns have taken a gradual and graceful curve, probably in
consequence of some slow movement impressed upon the lava
matter during the process of consolidation." The last merely
repeats the notion expressed by Bakewell (Introduction to Geo-
logy, 1st edit. 1813, p. 112), who says, " In some situations
basaltic columns are bent, apparently by the force of incumbent
pressure when in a soft and yielding state."

The first of these notions is devoid of any value, resting, as it
does, upon the false hypothesis originated by Gregory Watt,
that prismatic basalt has been produced by the squeezing toge-
ther of spheroidal lumps ; while the two latter are untenable on
the ground already proved, that the splitting into prisms can-
not commence until after the mass has been cooled enough to
become rigid. Curved prisms are frequently jointed ; and had
they been formed originally straight, and subsequently bent by
pressure, however slowly applied, after the mass had become
rigid, it is scarcely conceivable that transverse fractures of the
prisms should not be found, and that the transverse joints, if
produced in the way explained in the preceding pages, should
not have been opened upon the convex side of the curve ; indeed
the most natural supposition would be that the bending-force
would dislocate the straight and rigid prisms at the joints and
form bundles of straight prisms meeting each other at very ob-
tuse angles. It is not to be denied, however, that severe pres-
sure has in some instances been transmitted through masses of
prismatic basalt sufficient to generate a distinct slaty cleavage
in the prisms after their production from previously homoge-
neous material. Thus, in the neighbourhood of Mont d'Or les
Bains is a huge isolated mass of prismatic basalt, all the sepa-
rate joints of which have a slaty cleavage, so that they may be
split into a coarse sort of roofing-slabs, whence the mass
takes its local name of " la tuillerie." The pressure has varied
in direction in different parts of the mass, the prisms at one side
of which possess a cleavage approaching to a plane transverse to
their axes, while at the remote side of the mass the plane of
cleavage is inclined at a small angle to the axes. But there are
no evidences that the severe pressure sufficient to produce this
slaty cleavage has bent or distorted the prisms themselves—thus
presenting an additional proof that the basaltic mass was already
rigid when the splitting into prisms took place, and that the

rigidity was sufficient to prevent the subsequent distortion in the forms of the prisms without preventing those minute molecular movements by which slaty cleavage is developed in rigid bodies under severe pressure.

We have thus traced the production of prismatic structure, and all the leading phenomena which it presents in nature, to a single and simple cause, namely contraction by cooling, the admitted physical laws governing which sufficiently account for the hexagonal as the normal form of the prisms, for the diameters assumed by the prisms, for the variably cupped form and the direction taken by the convexity of the transverse joints and the variability of their distances apart, for the various directions in which the axes of straight prisms are found in nature, and for the production of prisms whose axes are variously curvilinear.

Upon the principle, then, that that solution of any natural problem which accounts, upon admitted principles, for all the phenomena, and introduces as consequences no new facts not accountable for, must be the true one, the writer ventures to believe he has solved this problem which for nearly ninety years has perplexed physical geology.

If the principles here enunciated be admitted, it will be readily seen that they present a powerful and valuable guide to the field-geologist for deciphering the hitherto perplexing phenomena presented by basalt, as well as many of those produced by it upon adjacent rocks.

It would seem therefore unnecessary to enter upon any formal refutation of those very crude and ill-thought-out notions which attribute prismatic structure to the squeezing together of globular or spheroidal masses like the "onion-stone" of Antrim, or like the superimposed spheroids which are to be found in all large basaltic regions. Such spheroids are found not confined to basalt, but existing as the residues of chemical decomposition and weathering in all rocky formations, presenting a more or less distinct horizontal stratification with vertical planes of separation. Such instances are familiar, not only in granitic, gneissose, and porphyritic rocks, but in many of a far softer character, argillaceous or calcareous. The superposed columns of orbicular masses of onion-stone prove themselves by every indication to be the decomposing remains of what were once jointed prismatic columns of basalt.

The exterior of each spheroid is friable or incoherent, coherence increasing as we advance toward the centre of the spheroid, which presents no nucleus of foreign matter or any indication that the spheroid has been produced by what have been vaguely called concretionary forces. The little irregular pris-

matic frusta which can be readily detached in crusts or singly from the exterior of the spheroids, are very generally separated by thin interposed surfaces of soft clayey material (kaolin), which has obviously been produced by the decomposition of the felspar, one of the chief constituents of basalts, as is proved by the marked deficiency of alkalies, found by analyses, in the exterior fragments of the spheroids, as compared with the proportion of these in the harder and deeper interior of the lump.   From the nature of the contractile forces of cooling, by which the prismatic joints or cheeses of basalt have been produced, as described in the preceding pages, it follows that some residual internal strains due to the final play of the contractile forces, and after those which have produced the prismatic and jointed structure have ceased to act, must exist in every one of these prismatic segments, producing, in a material so rigid as is basalt, interior planes of strain and of weakness in directions both circumferential and radial (as indicated in fig. 23), and assuming the spheroidal form the more nearly as they approach the centre.

FIC .23
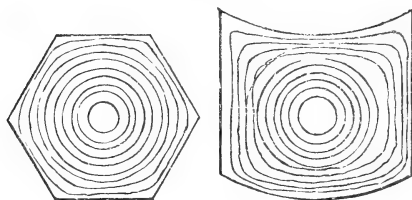
These planes of strain and of weakness become, under the influence of the chemical action of air and water, of rain and of frost, the planes of separation in which the prismatic segments become broken up, angles and corners disappearing first, until at last an irregular orbicular lump alone remains, or several of these superimposed.   And where these lumps are found not regularly superimposed, but thrown together *pell mell,* as often happens, a careful examination of the directions in which the planes of disintegration are found in the lumps occupying different parts of the mass is sufficient to prove that they have been formed from what were once prismatic segments or cheeses, detached and overthrown by mechanical forces from their pristine columnar arrangement.

Did space allow, much additional evidence might be adduced in proof of the fact that prismatic basalt has not been formed by any process whatsoever from such spheroids, but that, on the contrary, these latter have been produced by decomposition and disintegration of the pointed pieces of basaltic prisms previously

formed. The facility with which certain varieties of basalt decompose and disintegrate is instanced by Bakewell, who, in his work already quoted (p. 127), states that he had "seen a mound formed of basalt that had been got out of a mine by blasting with gunpowder, and which, he was informed, was once extremely hard and resisted the point of the pick; but by exposure to the air for thirty years it was converted into a rich mould and covered with a luxuriant crop of vegetables." Similar striking differences in facility of decomposition and disintegration have long been remarked in lavas. Torrents are to be found in abundance which, though poured forth beyond the reach of history or tradition, still resist decomposition, and scarcely permit even a lichen to grow upon their austere and rigid surface, while other examples are abundant of lavas largely decomposed into vegetable earth within a few generations of men; these facts tend to account for the presence of compact columnar basalt, and basaltic columns decomposed into onion-stone, existing in close proximity to each other.

The notion of production from spheroids is traceable to the paper of Mr. Gregory Watt, in the Phil. Trans. for 1804, entitled "Observations on Basalt," &c. Partly from the backward state of physical and chemical science as applied to geology at that period, this paper at once acquired an exaggerated reputation and currency, and has continued almost unchallenged for seventy years. So far, however, as relates to the production of prismatic basalt in the way assigned by Mr. Watt, it is impossible to avoid stating that his opinions rest upon a bad or imperfect experiment inaccurately reasoned upon and falsely applied. He fused in a reverberatory furnace (whether an iron-foundry " melting-furnace " or a " puddling-furnace " is not clear) some six or seven hundredweight of " Rowley-rag " basalt; and finding that after slow cooling some parts of the mass consisted of irregular spheroidal lumps of various sizes separated from each other by more or less flattened surfaces, which were red or rusty in colour, he concludes that by fusing and slow cooling this concretionary structure had resulted; he also infers that a like structure upon a much vaster scale must have been developed in all prismatic basalt, the prismatic structure being produced by the squeezing together of these concretionary lumps into the prismatic form. Now the immediate result of Mr. Watt's experiment was not fused basalt alone, but that material heterogeneously mixed up with the fused brick of his furnace (roof as well as sole), and most probably also with the silicates and oxides of iron adherent to it from former use, and more or less of the ashes of the fuel; hot sand moreover was employed to cover over the surface of the liquefied mass, so as to cause it to cool more slowly. All these conditions sufficiently show that the

concretionary structure was due to the heterogeneity of the material and the foreign nuclei introduced. The formation of such curviform lumps is familiar to every ironmaster as occurring occasionally in almost every variety of slag; but it is always traceable to heterogeneity or to abrupt and unequal cooling. Where is the evidence for any such concretionary development in the vast masses of prismatic basalt, which are amongst the most perfectly homogeneous of known rocks? But assuming that such a general structure did preexist, and that we had some evidence in support of the purely arbitrary assumption that some extraneous force existed sufficient to squeeze these to any extent of mutual distortion, where is the evidence that such mutual compression could even result in their being squeezed into continuous jointed prisms. The supposed concretionary masses can only be assumed thrown confusedly together like cannon-shot of various calibres in a pile, not even in regular layers or resting on any symmetric base; and as we ascend, the spheroids of each succeeding layer must drop, so far as the variability of their sizes will admit, to the lowest possible point between the supporting spheroids below. Each spheroid, therefore, as in the two only possible arrangements of shot in a pile, which may have a triangular or quadrangular base, must be in contact either with three or with four supporting spheroids below it; but in this case, which is in reality the only possible one, mutual compression (equal in all directions) will not produce prisms at all, but merely an aggregate of rhombic dodecahedrons differently posited in the two cases. If we are to have prisms as the result, the spheroids must be assumed arranged in vertical or rectilineal columns, each touching only one below it, in unstable equilibrium and in defiance of all the laws which regulate the positions of free and mutually attractive molecules or gravitating spheroids.

If we admit these enormous improbabilities, then prisms either tetrahedral or hexahedral might result from mutual compression; but, unless the spheroids were all precisely of equal diameter and identical in compressibility and retained exactly their original lineal arrangement during the whole progress of compression, the prisms could not be even approximately of equal diameter throughout. Nor could the cup-form of the cross joints ever be produced at all, whether, as Mr. Scrope imagines, the convex surfaces were always presented downwards, or the contrary; for it may be easily shown that the mutual distortion of such lines of spheroids into prisms, whether square or hexagonal, must result not in mutually fitting convexities and cups, but in extremely complicated opposite surfaces undulated both in radial and circumferential directions.

Such are the conditions of mutual compression which most writers on the subject since Mr. Watt have had somewhat confusedly in their minds (see Scrope, 'Volcanos,' 2nd edit. footnote p. 102 *et seq*.). But the conditions assumed by Mr. G. Watt are even still more far-fetched and chimerical, and may be stated in his own words :—" In a stratum composed of an indefinite number in superficial extent, but one only in height, of impenetrable spheroids with nearly equidistant centres, if the spheroids should come in contact on the same plane, it seems obvious that their mutual action would form them into hexagons ; and if these were resisted below and there was no opposing cause above them, it seems equally clear that they would extend their dimensions upwards, and thus form hexagonal prisms whose length might be indefinitely greater than their diameters " (Phil. Trans. 1804, Part i. p. 307). This supposition leads to even more extravagantly improbable results than the former*. None of these authors condescends to inform us whether their spheroids under compression are to be supposed cold and rigid, or heated so as to be plastic. We may infer from the context, however, in the case of Mr. Watt, if not also in that of Mr. Scrope, that each spheroid is in a state of plasticity by heat, thus yielding to pressure as an imperfect liquid. Vertical prisms are recorded by authors 150 feet in length and only 9 inches in diameter (Scope 'Volcanos,' p. 102), and others 6 to 8 feet in diameter and 40 feet and upwards in length. If we assume a prism of the smaller of these diameters and only 100 feet in length, it would demand a sphere for its production in the supposed manner equal to its own volume, or 4·1 feet in diameter, and, in the case of the larger diameter and length of prism, of 8·4 feet diameter. No instance has been observed, or probably exists, of any extensive collection of spheroids of either of these large sizes, or even approximately all of equal diameters. Admitting, *argumenti gratiâ*, the existence of some unknown external compressive force sufficient to overcome all the resistances, including that of the hydrostatic pressure of the

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* Mr. Watt's paper presents one of those singular examples, of which so many others exist in the records of scientific research, of how near an author may approach to completely discerning a truth and yet be blinded by a preconceived and falsely based hypothesis. At page 311 (Phil. Trans. as above) it will be seen that Watt had some clear ideas as to the splitting up into hexagonal prisms by contraction in cooling ; but immediately after (pp. 313–314) he dismisses this all in the words, "though these considerations may be sufficient to explain the tendency to division into prisms which is so generally extended . . . . they are utterly inadequate to the formation of the more perfect basaltic prisms ; they offer no means of accounting for the extreme regularity of the sides and the precision of the angles, for the articulations, for the close contact in which the perfect columns are placed to one another," &c.

forced-up columns, let us in one or two instances remark the consequences in which the assumed mechanism must land us. In the case of the 9-inch prism, let us suppose a surface of spheres of 4·1 feet diameter squeezed orthogonally and equally in both directions between the exterior edges and the centre; or in half a mile there are 640 spheres at the commencement which are squeezed by upward yielding into the same number of hexagonal prisms of 9 inches minor diameter, the aggregate of their diameters in the one line being $640 \times \cdot 75$ feet $= 480$ feet, deducting which from 2640 feet (half a mile), we find that the centre of figure of the exterior sphere must travel or be forced over the ground upon which they all rest by a distance equal to 2160 feet.

Is it possible that such an enormous range of transport should ever have occurred in nature and left no trace upon and beneath the base of the basaltic mass?

Compressive forces of enormous energy are to be found in nature, but always acting through *small* ranges. Where have we any examples of such forces acting through such immense ranges as are here required and yet leaving no trace of their existence? Assuming, however, that such have so acted, the following would be amongst the consequences of which evident trace could not but be found:—

1. The compressive effect in elevating the squeezed-up columns will not be uniform along any one line of spheres, but will be greatest upon those spheres which are nearest to the exterior of the surface pressed; the final effect, therefore, of such compression upon a square mile of spheres 4·1 feet diameter will not be to produce a flat-topped tabular mass of $480 \times 2 = 960$ feet square, but one having the general form of an inverted square pyramid, the prisms being tallest all round the outside and of least altitude in the centre. Has any such general form, or any thing even remotely resembling it, in large masses of vertically prismatic basalt ever been observed? On the contrary, do not all such masses present the general characteristic of having nearly level or more or less irregularly convex tops?

2. If the prisms be produced by the squeezing against each other of a stratum of spheres plastic by heat, then must internal movements have been produced in the mass of each sphere during its distortion, resulting in a distinct molecular structure or arrangement of the particles of the prism throughout its length, from which it would result that the prism itself would be more difficultly frangible transverse to its axis (the only direction in which joints are found to occur), and be most easily frangible or cleavable in planes parallel to the sides of the prism, or in

radial planes passing through its axis—contrary to the result of observation in all natural basalt. Were it worth while to occupy space in further refutation of this notion, it would be easy to point out many other incongruous conditions either involved in the conception or in its consequences. One other notion is to be found more or less obscurely suggested by various authors, and to which even some countenance has been given by Delesse; it is that which supposes basaltic prisms to have resulted from crystallogenic forces, and to be themselves of the nature of gigantic crystals.

The mere magnitude of the prisms is not alone sufficient to overthrow this notion; for undoubted crystals of quartz, of beryl, carbonate and sulphate of lime, galena, common salt, &c. are known to exist of magnitudes vying with the diameters at least of basaltic prisms. But these all possess the character of true crystals, viz. that their internal structure is throughout that announced by their exterior contour; but in no basaltic prism, however distinctly crystallized may be the integrant particles of dissimilar minerals (pyroxene, felspar, &c.) of which the prism consists, is there the slightest relation traceable between the directions of the axes of these integrant crystals and the axis or contour of the prism itself. We might as well call a hexagonal column of granite or porphyry cut out by the hand of a lapidary a crystal, as to apply the term to a basaltic prism.

But though crystalline forces have had no part in the production of basaltic prisms, the laws of their production by symmetric fracturing, due to cooling and contraction, do present some interesting analogies with the results of crystallization under certain circumstances in bodies solidifying from fusion.

It has been pointed out by the writer, in his paper " On the Physical Conditions involved in the Construction of Artillery " (in the 'Transactions' of the Royal Irish Academy, vol. xxii. June 1855), that in all such cases the principal axes of the crystals constituting the cooled mass arrange themselves in directions orthogonal to the external contours of the mass—that is, in the directions in which the heat-wave has passed from the cooling body; that is to say, the principal axes of the integrant crystals are in their arrangement very similar to those of the axes of the prisms in prismatic basalt. And the analogy extends even to this—that the principal axes of the crystals are curved where the crystallizing mass has curved surfaces of contour, or where the volume in relation to the cooling surface changes more or less abruptly; in other words, where the isothermal surfaces of cooling are divergent, as, for example, in the ogival portion of elongated projectiles for rifled cannon of chilled cast iron; the principal axes of the crystals are found curved, as in fig. 24,

and in the same way as described in the preceding pages with respect to the curved prisms of basalt, and illustrated by fig. 22 B, which may be viewed as a longitudinal section of one half of the projectile, B B being its axis. But these appear to be mere

Fig. 24.
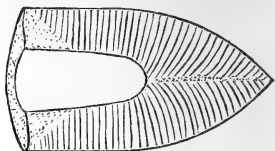
resemblances and analogies; and if there is any physical connexion between the splitting forces by which prismatic basalt has been produced and those crystallogenic forces first pointed out by the writer in 1855 and here referred to, it will be hereafter to be sought for by the laws ascertained by Mitscherlich with respect to the different dilatation (or contraction) of crystals in different directions with respect to their principal axes, which seem to point to the conclusion that the formation of the crystalline planes of separation in masses of crystallizable substances as they cool may be due to differences of contractility of their constituent crystals in different directions.   If so, the production of crystalline planes of separation parallel to the principal axes in a mass crystallizing from cooling may prove to be as much the result of contraction as the splitting-up of an uncrystallizable mass of basalt into prisms is so.   All bodies which contract by heating, by cooling, and by drying, such as sand baked into stone, starch, mud, coke, &c., tend to separate, as has been often remarked, into more or less prismatic masses; upon the particular conditions of which in different cases it is not necessary here to enlarge, further than to remark that in the case of mud, which as it occurs in nature has almost invariably been deposited from water in shallow hollows or cavities, the deposit thins out towards its edges.   In drying, therefore, by sun and air, the stratum is observed to crack first at and near the edges and in directions more or less orthogonal to these.   The irregular ribbons thus produced between two contiguous cracks again split transversely by further drying into more or less square pieces—a form which thus results from two successive stages of contraction, in contradistinction to the one which produces the hexagonal division of the basalt, as enunciated in the preceding pages.   It must be borne in mind that the contractile forces produced by baking, desiccation, &c. must act locally and in succession on different parts of the mass, and can never produce the same regularity of separation as is found in a mass of cooling basalt.

XXV. *On an Alleged Error in* Laplace's *Theory of the Tides.*
By Sir WILLIAM THOMSON, *F.R.S.**

1. AIRY, in his article on Tides and Waves in the *Encyclopedia Metropolitana*, gives a version of Laplace's theory of the tides which has the undoubtedly great merit of being freed from certain inappropriate applications of " Laplace's co-efficients," or, as they are now more commonly called, " Spherical harmonics," by which its illustrious author attempted, not quite successfully, to design a method for taking into account the alteration of gravity due to the tidal disturbance of the surface of the sea in the solution of his dynamical equations.

It is the repetition of this attempt for each of the " three species of oscillations " (*Mécanique Céleste*, Liv. iv. arts. 5., 7., 9.), and the preparation for it [in the course of working out the fundamental differential equations (art. 3.)] by the assertion that " a, b, c, a' are to be rational functions of μ and $\sqrt{1-\mu^2}$," that throws a cloud over nearly the whole chapter (Liv. iv. chap. i.) of the *Mécanique Céleste* devoted to the dynamical theory of the tides, and fully justifies the following statement with which Airy [Tides and Waves, art. (66)] introduces his own version of Laplace's theory :—

" It would be useless to offer this theory in the same shape in " which Laplace has given it ; for the part of the *Mécanique* " *Céleste* which contains the Theory of Tides is perhaps on the " whole more obscure than any other part of the same extent " in that work. We shall give the theory in a form equivalent to " Laplace's, and indeed so nearly related to it that a person fami- " liar with the latter will perceive the parallelism of the successive " steps. The results at which we shall arrive are the same as " those of Laplace."

2. The only good thing lost in Airy's treatise through the omission of the spherical harmonic analysis is Laplace's complete solution for the case of no rotation and equal depth of the sea all round the earth. When the earth's rotation is taken into account, or when the sea is of unequal depth, the differential equation to be solved takes a form altogether unsuited for the introduction of spherical harmonics; and Airy's investigation is substantially the same as Laplace's, except the judicious omission of the unsuccessful attempts referred to above.

3. In giving Laplace's solution for the semidiurnal tide with the change of gravity due to the change of figure of the water not taken into account, Airy points out what he believed to be an error, so serious that, after correcting it, it was " needless to

"observe that Laplace's numerical calculations of the heights of "tides in certain latitudes, and his inferences as to the latitude "where there is no tide &c., fall to the ground." When I first read Airy's treatise ten years ago on board the 'Great Eastern,' I could not assent to his correction of Laplace, but, on the contrary, satisfied myself that Laplace was quite right. Not having the *Mécanique Céleste* at hand, I set the subject aside for a time, intending to return to it for the second volume of 'Thomson and Tait's Natural Philosophy,' with the first volume of which I was then occupied.

4. My attention has recently been recalled to it by reading in a volume of 'Tidal Researches,' constituting an appendix contributed by W. Ferrel to the 'United States Coasts Survey Report for 1874,' the following passage referring to Laplace's solution for the semidiurnal tides:—

"The results show that the form which the surface of the "sea assumes in this case differs very much from that of a prolate "spheroid with its longer axis in the direction of the disturbing "body, and that for certain depths of the ocean the tides at the "equator are inverted, low water taking place under the attracting "body. For great depths, however, the tides were found to be "direct in all latitudes; and even in the cases in which they "are inverted at the equator they were found to be direct to-"ward the poles, and consequently there is a latitude in such "cases where there are no tides. Laplace, however, failed to in-"terpret correctly in this case his own solution, so that the nume-"rical results which he has given for different assumed depths of "the ocean are erroneous; but still the general results just stated "are readily seen from the solution. Having failed to see the in-"determinate character of the problem, he adopted a singular and "unwarranted principle for determining the value of a constant "which is entirely arbitrary in the case of no friction, but which "vanishes in the case of friction, however small. This oversight "of Laplace and the indeterminateness of this constant were sub-"sequently pointed out by Airy." The "singular and unwar-ranted principle" thus referred to is in fact an exquisitely subtle method by which it seems Laplace had determined a constant which is not arbitrary in any case, and which cannot be more than infinitesimally modified by infinitesimal friction. Ferrel further extends to Laplace's integration for the "diurnal tide" the objection of indeterminateness which Airy had raised only against his integration for the semidiurnal; and he follows Airy in an integration (not given by Laplace) for the "long-period tide," in which a false appearance of determinateness (strangely inconsistent with the indeterminateness asserted of the solutions for the semidiurnal and the diurnal) is produced

by the inadvertent omission of a constant*, the true value of which is to be determined by a proper application of Laplace's method. With these results before me, I cannot wait two or three years more for the second volume of 'Thomson and Tait's Natural Philosophy' to defend Laplace's process, but must speak out on the subject without delay; and therefore I offer the present article for publication in the Philosophical Magazine, regretting that I did not do so ten years earlier.

5. To Airy's statement of his case against Laplace, quoted in full below, I premise, by way of explanation :—

I. The tide-generating force for the case in question (the "semidiurnal tide") is such that the equilibrium-tide height is represented by the formula

$$\text{H} \sin^2 \theta \cos 2\phi, \text{ or } \text{H}x^2 \cos 2\phi, \quad . \quad . \quad . \quad (1)$$

where H is a constant, θ is the colatitude of the place, or x the cosine of the latitude, and ϕ the hour-angle of the disturbing body, which may be conveniently supposed to consist of moon and anti-moon (two halves of the moon's mass) placed opposite to one another at distances equal to the moon's mean distance from the earth in a line kept always in a fixed direction through the earth's centre and in the plane of the equator.

II. Instead of H $\sin^2 \theta$ or Hx^2 in the equilibrium formula, put a, so that it becomes $a \cos 2\phi$. This expresses the actual tide-height if a be a function of the latitude fulfilling over the whole surface the differential equation (*Méc. Cél.* Liv. iv. art. 10.)

$$(1-x^2)x^2 \frac{d^2a}{dx^2} - x \frac{da}{dx} - 2\left(4 - x^2 - \frac{2mr}{\gamma}x^4\right)a = -8\text{H}x^2, \quad (2)$$

where m denotes the ratio of centrifugal force to gravity at the earth's equator (its value being actually about $\frac{1}{289}$), r the earth's radius, and γ the depth of the sea, in the present case assumed to be uniform all over the earth's surface, and but a small fraction of the radius.

III. Remark that the period of the disturbance thus investigated is rigorously half the period of the earth's rotation—that is to say, half the sidereal day. This supposition is no doubt quite a close enough approximation for the solar semidiurnal tide; but it is certainly not practically close enough for the lunar semidiurnal tide, its period exceeding, as it does, the half-sidereal day by about $\frac{1}{28}$ of its value.

IV. Remark also that if the earth's rotation is infinitely slow, m is infinitely small, and the differential equation is satis-

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* See an article in the next (October) Number of the Phil. Mag., entitled "Note on the Oscillations of the First Species in Laplace's Theory of the Tides."

fied by

$$u = \mathrm{H}x^2;$$

that is to say, agreement with the equilibrium tide.

V. Lastly, Laplace and Airy assume

$$u = \mathrm{K}_2 x^2 + \mathrm{K}_4 x^4 + \ldots + \mathrm{K}_{2k} x^{2k} + \mathrm{K}_{2k+2} x^{2k+2} + \&\mathrm{c}. \quad . \quad . \quad (3)$$

as a solution of the differential equation (2). Then, by equating the coefficient of x^2 in the left-hand member to $-8\mathrm{H}$, its coefficient in the right, and by equating to zero the coefficient of x^{2k+4} for all values of k from 0 to ∞, they find

$$-2 \cdot 4\mathrm{K}_2 = -8\mathrm{H} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

and

$$2k(2k+6)\mathrm{K}_{2k+4} - 2k(2k+3)\mathrm{K}_{2k+2} + \frac{4mr}{\gamma}\mathrm{K}_{2k} = 0 \quad . \quad (5)$$

for all positive integral values of k [the case of $k=0$ justifies the omission of K_0 in (3)]. The first of these equations of condition gives $\mathrm{K}_2 = \mathrm{H}$. The second, if for brevity we put $\frac{mr}{\gamma} = e$, gives,

$$\mathrm{K}_{2k+4} = \frac{2k+3}{2k+6}\mathrm{K}_{2k+2} - \frac{e}{k(k+3)}\mathrm{K}_{2k}, \quad . \quad . \quad . \quad (6)$$

and so determines successively K_6, K_8, K_{10}, . . . &c., all in terms of K_2, K_4. Thus the differential equation (2) is satisfied by (3) with K_2 given by (6), K_4 arbitrary, and the other coefficients given by (6).

6. On this and Laplace's process for completing the solution Airy [art. (111)] remarks :—

"The indeterminateness of K_4 is a circumstance that admits of "very easy interpretation. It is one of the arbitrary constants in "a complete solution of the equation. It shows that we may give "to K_4 any value that we please, even if $\mathrm{H}*=0$; and then, pro- "vided that we accompany our arbitrary K_4 with the correspond- "ing values of K_6, K_8, &c., we shall have a series which expresses "a value of a † that will satisfy the equation *when there is no ex-* "*ternal disturbing force whatever*, and which therefore may be "added, multiplied by any number, to the expression determined "as corresponding to a given force. In the next section we shall "find several instances exactly similar to this. Yet this obvious "view of the interpretation of this circumstance appears to have

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* G in Airy's notation, $\dfrac{2\mathrm{L}}{4r^3 g}$ in Laplace's.

† ga in Airy's notation, αa in Laplace's.

" escaped Laplace, and he has actually persuaded himself to
" adopt the following process. Putting the general equation
" among the coefficients into the form

$$\frac{K_{2k+2}}{K_{2k}} = \frac{2\,\dfrac{bm}{l}}{(2k^2+3k)-(2k^2+6k)\,\dfrac{K_{2k+4}}{K_{2k+2}}},$$

" he has unwarrantably conceived that this must apply when $k=1$
" for the determination of K_4; and thus applying the same equa-
" tion to each quotient of terms which occurs in the denominator
" of the fraction, he finds

$$\frac{K_4}{K_2} =$$

$$\cfrac{2\,\dfrac{bm}{l}}{2.1^2+3.1-(2.1^2+6.1)\times \cfrac{2\,\dfrac{bm}{l}}{2.2^2+3.2-(2.2^2+6.2)\times \cfrac{2\,\dfrac{bm}{l}}{2.3^2+3.3-\&c.}}}$$

" in an infinite continued fraction. And upon this he founds
" some numerical calculations adapted to different suppositions of
" the depth of the sea. We state, as a thing upon which no person
" after examination can have any doubt, that this operation is en-
" tirely unfounded."

7. A careful examination at the time when I first read this
led me to the opposite conclusion, and showed me that Laplace
was perfectly right. If $\dfrac{K_{2k+2}}{K_{2k}}$ vanishes when k is infinitely
great, then $\dfrac{K_4}{K_2}$ cannot but be equal to the continued fraction.
What, then, must be the case if K_4 has any other value than that
determined by Laplace? $\dfrac{K_{2k+2}}{K_{2k}}$ cannot then converge to zero
for greater and greater values of k. But unless $\dfrac{K_{2k+2}}{K_{2k}}$ is infi-
nitely small when k is infinitely great, the second term of the
second member of (6) is infinitely small in comparison with the
first, and therefore ultimately

$$K_{2k+4} = \frac{2k+3}{2k+6}\,K_{2k+2},$$

or

$$K_{2k+2} = \frac{2k+1}{2k+4} K_{2k},$$

$$= \left(1 - \frac{3}{2k}\right) K_{2k},$$

when k is infinitely great. Now this is precisely the degree of ultimate convergence of the coefficients of x^{2k}, x^{2k+2}, &c. in the expansion of $(1-x^2)^{\frac{1}{2}}$. Hence, when x is infinitely nearly equal to unity, a is finite, and so also is $\sqrt{(1-x^2)}\frac{da}{dx}$, or $\frac{da}{d\theta}$. Now clearly at the equator (or when $x=1$) we must have $\frac{da}{d\theta}=0$, because of the symmetry of the disturbance in the northern and southern hemispheres in the case proposed for solution by Laplace and Airy. Hence in this case $\frac{K_{2k+2}}{K_{2k}}$ must converge to zero, and therefore K_4 must have the value given to it by Laplace.

8. Look now to the degree of convergence obtained by Laplace's evaluation of K_4 and verify that it secures $\frac{da}{d\theta}=0$ when $\theta=\frac{1}{2}\pi$. Put

$$\frac{K_{2k+2}}{K_{2k}} = R_k. \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

By this we have

$$K_{2k+4} = R_k \cdot R_{k+1} \cdot K_{2k};$$

and (6) resolved for R_k gives

$$R_k = \frac{\dfrac{e}{k(k+3)}}{\dfrac{2k+3}{2k+6} - R_{k+1}}. \quad . \quad . \quad . \quad . \quad (8)$$

Hence, unless the ratios converge to unity, (8) gives

$$R_k = \frac{2e}{k(2k+3)} \quad \text{when } k \text{ is great.} \quad . \quad . \quad . \quad (9)$$

Now Laplace's determination of K_4 by his continued fraction implies the determination of the ratios by taking $R_{k+1}=0$ for some very great value of k and calculating

$$R_k, \quad R_{k-1}, \quad R_{k-2}$$

by successive applications of (8) with $k-1$, $k-2$, ... substituted for k. Hence it gives to the series (3) a degree of convergency (approximately the same as that of the expansion of $\epsilon^{x\sqrt{e}} + \epsilon^{-x\sqrt{e}}$ in

powers of x, and) such that $\dfrac{da}{dx}, \dfrac{d^2a}{dx^2}, \dfrac{d^3a}{dx^3}, \ldots$ &c. are all finite for every finite value of x. Hence $\dfrac{da}{d\theta}$, being equal to $\sqrt{(1-x^2)}\dfrac{da}{dx}$, is zero when $x=1$.

9. Thus it appears that Laplace's process simply determines K_4 to fulfil the condition that $\dfrac{da}{d\theta}=0$ at the equator. And the assumed form of solution (3) has the requisite convergency to zero when $x=0$, for the poles. Laplace's result is therefore the solution of the determinate problem of finding the tidal motion in an ocean covering the whole earth continuously from pole to pole. *Whatever other motion the sea could have in virtue of any initial disturbance cannot, except for certain critical depths, have the same period as that of the assumed tide-generating force.*

10. If the sea be precisely of such a depth that some one of the possible free vibrations in which the height of the surface at any instant is expressible by the formula $v\cos 2\psi$, where ψ denotes longitude, and v some function of the latitude having the same value for equal north and south latitudes, has its period equal to that of the tide-generating influence, it is easily seen that the solution of the differential equation (2) gives an infinitely great value to a. It is only when the depth has one of these critical values that the arbitrary solutions introduced by Airy and adopted by Ferrel are applicable to an ocean covering the whole earth continuously.

11. Yet Laplace himself fell into the same error of imagining that the general integration of the differential equation (2), with the proper arbitrary constants, includes oscillations depending on the primitive state of the sea, as the following passage (Liv. iv. chap. i. art. 4) shows :—

"L'intégration de l'équation (4)[*] dans le cas général où n n'est "pas nul, et où la mer a une profondeur variable, surpasse les "forces de l'analyse ; mais pour déterminer les oscillations de "l'océan, il n'est pas nécessaire de l'intégrer généralement ; il "suffit d'y satisfaire ; car il est clair que la partie des oscillations "qui dépend de l'état primitif de la mer, a dû bientôt disparaître "par les résistances de tout genre que les eaux de la mer éprou-"vent dans leurs mouvemens ; en sorte que sans l'action du soleil "et de la lune, la mer serait depuis longtemps parvenue à un état "permanent d'équilibre : l'action de ces deux astres l'en écarte "sans cesse, et il nous suffit de connaître les oscillations qui en "dépendent."

* A general equation, of which equation (2) of our numbering above is a particular case.

Laplace, however, did not suffer himself to be led into wrong action by this misconception, and he seems to have entirely forgotten it when he goes direct to the right result, without note or comment, by the truly "singular" process referred to above.

12. On the other hand, Airy, after having, in the passage quoted in § 6 above, allowed the same misconception to fatally influence his practical dealing with the solution, closes with a perfectly correct statement which is sufficient to show the groundlessness of his objection to Laplace's result, and the untenability of what he substitutes for it. This passage has not only the merit of inconsistency with the article which precedes it, but it also constitutes a very decided advance in the theory beyond any thing that Laplace either did or suggested, and for both reasons I am glad to quote it. [Airy, 'Tides and Waves,' art. (113).] "If, using the more complete values of a that we have "just found, we proceed to form the values of a''', b, and u, we find "that u* will contain a series of terms multiplied by the indeter- "minate K_4. We may determine K_4 so that, for a given value of "θ, u shall $= 0$; that is to say, so that, in a given latitude, the "water shall have no north-and-south motion. We might there- "fore suppose an east-and-west barrier (following a parallel of lati- "tude) to be erected in the sea, and the investigation would still "apply. Thus, then, we have a complete solution for a sea which "is bounded by a shore whose course is east and west."

13. Now in fact Laplace's process by the continued fraction is a particular case of the determination of K_4 thus suggested by Airy, though one for which Airy's method fails through nonconvergence; that is to say, the case in which the proposed east-and-west barrier coincides with the equator. For as we have seen (§ 8), Laplace's determination makes $\frac{da}{d\theta} = 0$ when $\theta = \frac{1}{2}\pi$, and therefore makes the north-and-south motion zero at the equator, as is obvious from symmetry, or as we see from the general expression [Laplace, Liv. iv. art. 3; or Airy, arts. (85), (95)]

$$u = \frac{1}{4m\sin^2\theta}\left(\frac{da}{d\theta} + \frac{2\cos\theta}{\sin\theta}\,a - 4\mathrm{H}\sin\theta\cos\theta\right)\cos 2\phi \quad . \quad (10)$$

for the southward component of the displacement of the water by the semidiurnal tide.

14. By § 7 above we see that Laplace's solution (3), with K_4 left arbitrary, is convergent for all values of $x < 1$. Therefore it is continuously convergent for all values of $\theta < \frac{1}{2}\pi$. Hence Airy's article (113), with the formulæ which he gives in his

* This denotes the meridional component of the displacement of the water in any part of the sea.

article (112), or equations (3) and (4) and (10) of §§ 5 and 13 above, constitute a complete and convergent numerical solution of the problem of finding the semidiurnal tide in a polar basin, or ocean continuous and equally deep from either pole to a shore lying along any circle of latitude on the near side of the equator. Laplace's result, as we have seen, does the same for a hemispherical sea from pole to equator. But for a sea extending from either pole to a coast coinciding with a circle of latitude beyond the equator another form of solution (still, however, with but one arbitrary constant) must be sought*, because Laplace's form [(3) of § 5 above] ceasing to converge when x (or the sine of the polar distance θ) increases up to unity, fails to provide for the continuous variation of θ from zero to any value exceeding $\frac{1}{2}\pi$. And, further, the method suggested by Airy, when extended to a complete solution of the differential equation with its two arbitrary constants†, completely solves the problem of finding the semidiurnal tide in a zonal sea of equal depth between coasts coinciding with any two parallels of latitude.

15. Returning to Laplace's solution for the whole earth covered with water, we find in the *Mécanique Céleste* the numerical results referred to by Airy (but not quoted, because of the supposed error in the process by which they were obtained). They are of exceedingly great interest (when we know them to be correct); and, in the circumstances, I may be permitted to quote them here. They are obtained by working out numerically the process indicated in §§ 5 and 6 above, for three different depths of the sea, $\dfrac{1}{2890}$, $\dfrac{1}{722\cdot5}$, $\dfrac{1}{361\cdot25}$ of the earth's radius. The values of e, or $\dfrac{mr}{\gamma}$ corresponding to these depths, are 10, 2·5, 1·25 respectively; and Laplace finds for the solution [(3) § 5] in the three cases as follows :—

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* It is to be found by using Laplace's first differential equation [the one from which he derives (2) of § 5 above by putting $1-\mu^2=x^2$ (Liv. iv. art. 10.)],

$$(1-\mu^2)^2\frac{d^2a}{d\mu^2}-2[3+\mu^2-2e(1-\mu^2)^2]a=-8\mathrm{H}(1-\mu^2),$$

and satisfying it by the assumption

$$a=\mathrm{A}_0+\mathrm{A}_1\mu+\mathrm{A}_2\mu^2+\ \&\mathrm{c.}\ ;$$

which, however, is a complete solution with two arbitrary constants, to be reduced to one by the proper condition to make $u=0$ at *one* pole (say, when $\mu=+1$).

† The general solution indicated in the preceding footnote suffices for this purpose.

$(e = 10)$, $a = H \{ 1 \cdot 0000 . x^2 + 20 \cdot 1862 . x^4 + 10 \cdot 1164 . x^6$

$$- 13 \cdot 1047 . x^8 - 15 \cdot 4488 . x^{10} - 7 \cdot 4581 . x^{12}$$

$$- 2 \cdot 1975 . x^{14} - 0 \cdot 4501 . x^{16} - 0 \cdot 0687 . x^{18}$$

$$- 0 \cdot 0082 . x^{20} - 0 \cdot 0008 . x^{22} - 0 \cdot 0001 . x^{24} \};$$

$(e = 2 \cdot 5)$, $a = H \{ 1 \cdot 0000 . x^2 + 6 \cdot 1960 . x^4 + 3 \cdot 2474 . x^6$

$$+ 0 \cdot 7238 . x^8 + 0 \cdot 0919 . x^{10} + 0 \cdot 0076 . x^{12}$$

$$+ 0 \cdot 0004 . x^{14} \};$$

$(e = 1 \cdot 25)$, $a = H \{ 1 \cdot 0000 . x^2 + 0 \cdot 7504 . x^4 + 0 \cdot 1566 . x^6$

$$+ 0 \cdot 01574 . x^8 + 0 \cdot 0009 . x^{10} \}.$$

By putting $x = 0$ in each case we find $a = 0$, showing that there is no rise and fall at the poles. Putting $x = 1$, we find in the three cases,

$$a = - 7 \cdot 434 . H \ldots (\text{depth } \frac{1}{2890} \text{ of radius}),$$

$$a = 11 \cdot 267 . H \ldots (\text{ , } \frac{1}{722 \cdot 5} \text{ , }),$$

$$a = 1 \cdot 924 . H \ldots (\text{ , } \frac{1}{361 \cdot 25} \text{ , }).$$

The negative sign in the first case shows that the tide is "inverted" at the equator; or there is low water when the disturbing body is on the meridian, and high water when it is rising or setting. For small values of x (that is to say, for polar regions) the sign is positive, and therefore the tides are direct for this, as clearly for every other depth (because in every case the first term is $+ Hx^2$). In the particular case in question (depth $\frac{1}{2890}$), as we see from the formula given above, the value of a increases from zero to a positive maximum, and then decreases to the negative value stated above as x is increased from 0 to 1; and the intermediate value of x which makes it 0 is roughly ·95, or the cosine of 18°. Hence Laplace concludes the tides are inverted in the whole zone between the parallels of 18° north and south latitude, while throughout the regions north and south of these latitudes the tides are direct. The formulæ given above for the second and third of the depths chosen by Laplace shows that in these cases the tides are everywhere direct and increase continuously from poles to equator.

The results of the summation for the equatorial tide in the three cases given above are very interesting as showing how much greater it is in each case than H (the equilibrium height). Upon this Laplace remarks that for still greater depths the value

of a diminishes; but this diminution has a limit, namely the equilibrium value, which it soon approximately reaches. To find what is meant by " soon " (" bientôt ") take the case of $e = \frac{1}{4}$, or depth $\frac{1}{72 \cdot 25}$ of the radius. For a rough approximation to R_3 take $R_4 = 0$, and use formula (9) § 8 with $k = 3$. Thus we have

$$R_3 = \frac{1}{54}.$$

Then by successive applications of (8) with $k = 2$ and $k = 1$ we find

$$R_2 = \cdot 0367 \text{ and } R_1 = \cdot 104.$$

Hence in this case we have roughly

$$a = H(x^2 + \cdot 104 \cdot x^4 + \cdot 104 \cdot \cdot 0367 \cdot x^6 + \cdot 104 \cdot \cdot 0366 \cdot \cdot 0185 \, x^8)$$
$$= H(x^2 + \cdot 104 \cdot x^4 + \cdot 00382 \cdot x^6 + \cdot 000071 \cdot x^8),$$

which shows that when the depth is about a seventieth of the radius, the actual amount of equatorial tide exceeds the equilibrium amount by nearly eleven per cent.

16. From the first and second of Laplace's numerical formulæ for a given above (§ 15), we may infer that when e is increased from $2 \cdot 5$ continuously to 10, the value of a for any value of x must increase continuously to $+ \infty$, then suddenly become $- \infty$, and increase continuously from that till it has the value given by the formula for $e = 10$. When e has a value exceeding by however small a difference the value which makes $a = \pm \infty$, the value of a for very small values of x is positive, and diminishes through 0 to very large negative values as x is increased to 1; that is to say, there are nodes coinciding with two very small circles of latitude, one round each pole, direct tides within these circles, and very great inverted tides round the rest of the earth. As e is increased continuously from this first critical value, the nodal circles expand until (as seen above) when $e = 10$ they coincide approximately with 18° North and South latitude. From the greatness of the coefficient of x^4 in Laplace's result for this case we may judge that e cannot be increased much above 10 without reaching a second critical value, for which the coefficient of x^4, after increasing to $+ \infty$, suddenly becomes $- \infty$. It is probable that the nodal circles do not get much nearer the equator than 18° North and South before this critical value is reached. When e is increased above it, a second pair of nodal circles commence at the two poles, spreading outwards and getting nearer to the former pair of nodal circles, which themselves are getting nearer and nearer to the equator. Then there are direct tides in the equatorial belt, inverted tides in the zones between the nodal parallels of

latitude in each hemisphere, and direct tides in the north and south polar areas beyond. This is the state of things for any value of e greater than the second critical value just considered and less than a third. When e is increased through this third critical value, a third pair of nodal circles grows out from the poles; and there are inverted tides at the equator, direct tides in the zone between the nodal circles of the first and second pair, inverted tides in the zones between the second and third nodal circles of each hemisphere, and still, as in every case, direct tides in the areas round the poles; a fourth critical value of e introduces a fourth pair of nodal circles, and so on.

17. The critical values of e which we have just been considering are of course those corresponding to depths for which free vibrations of the several types described are symperiodic with the disturbing force; and the free oscillations without disturbing force are in these cases expressed by the formula (3) of § 5, with $K_2 = 0$—that is to say, by

$$a = K_4 x^4 + K_6 x^6 + K_8 x^8 + \&c.,$$

where K_4, K_6, &c. are to be found by giving an arbitrary value to any one of them, and determining the ratios R_1, R_2, &c. by successive applications of Laplace's formula,

$$R_k = \frac{\dfrac{e}{k(k+3)}}{\dfrac{2k+3}{2k+6} - R_{k+1}}, \quad . \quad [(8) \text{ of } § 8]$$

with diminishing values of k, commencing with a value corresponding to the highest ratio to be used in calculating coefficients in the series. If we thus find $R_2 = \frac{5}{8}$, the next application of the formula gives $R_1 = \infty$, which is the test that the value of e used in the calculation corresponds to a depth for which the period of one of the free oscillations is exactly half the earth's period of rotation.

18. The calculation of the ratios R_1, R_2, R_3 is an exceedingly curious and interesting subject of pure mathematics or arithmetic. First, remark the rapid extinction of the error resulting from taking 0 or any other than its true value for R_{k+1} in the first application of the formula (8). Supposing k to be so large that $\dfrac{e}{k(k+3)}$ is a small fraction, we know that this is somewhat approximately the value of R_k, and that $\dfrac{e}{(k+1)(k+4)}$ is still more approximately the value of R_{k+1}. Hence we see at once how small the error is if we take 0 instead of R_{k+1}. If we take $\pm \infty$ for R_{k+1}, the formula gives 0 for R_k, and then rapid

convergence to the true values of R_{k-1}, R_{k-2}, &c. If we take R_{k+1} exactly equal to $\dfrac{2k+3}{2k+6}$, we get $R_k = \infty$, $R_{k-1} = 0$, and then rapid convergence to the true values for R_{k-2}, &c. But if we take for R_{k+1} a value less than $\dfrac{2k+3}{2k+6}$ by a certain very small difference, we find for R_k a value less than $\dfrac{2k+1}{2k+4}$ by a corresponding very small difference, and then for R_{k-1} a value less than $\dfrac{2k-1}{2k+2}$ by a corresponding small difference, and so on. Any value of R_{k+1}, except precisely the one particular value last indicated, will, provided k be large enough, lead to the desired values of the lower ratios after the two, three, four or more successive applications of the formula required to dissipate the effects of the initial error. It is a curious and instructive arithmetical exercise to calculate R_k, R_{k-1}, and so on down to R_1; and then by successive reverse applications of the formula to calculate R_2, R_3, ... R_{k-1}, R_k. If the calculation has been rigorous, of course the initial value of R_k will be that found at the end of the process; but if the calculation has been approximate (say, with always the same number of significant figures retained in each step), the value found for R_k will be not the initial value, but $\dfrac{2k+1}{2k+4}$, or, more approximately, $\dfrac{2k+1}{2k+4} - \dfrac{e}{(k-1)(k+2)}$. And if we choose for R_1 any other value than precisely that obtained by an infinitely accurate application of Laplace's process, then work up by successive reverse applications of the formula, we find for R_k a value approximately equal to $\dfrac{2k+1}{2k+4}$*. Laplace

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* Compare with the calculation of the formula

$$r_i = \frac{1}{2a - r_{i-1}},$$

where a denotes any numerical quantity > 1. Take any value at random for r_0, and calculate r_1, r_2, r_3 ... by successive applications of the formula. For larger and larger values of i, r_i will be found more and more nearly equal to the smaller root of the equation

$$x^2 - 2ax + 1 = 0.$$

Now calculate backwards to r_0 by the reversed formula

$$r_{i-1} = 2a - \frac{1}{r_i};$$

and instead of finding the initial value of r_0 again, the result (unless the calculation has been rigorously accurate in every step) will be approxi-

has not warned us of this; on the contrary, his instructions, literally followed, would lead us simply to calculate K_4 by his continued fraction and then to calculate K_6, K_8, &c. successively from K_2 and K_4 by successive applications of the formula (6) of § 5. This, except with infinitely rigorous arithmetic, will bring out for very large values of k not the true rapidly diminishing values of the coefficients K_{2k}, K_{2k+2}, &c., but sluggishly converging values corresponding to the ratio $R_k = \dfrac{2k+1}{2k+4}$. But this dissipation of accuracy is avoided, and at the same time the labour of the process is much diminished, by using for the ratios the values already found for them in the successive steps in the calculation of the continued fraction for R_1.

19. The law of variation of R_1, R_2, &c., considered as functions of e (§ 8), is of fundamental importance. Some of the remarkable characteristics which it presents have been already

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mately the *greater* root of the quadratic, or approximately equal to $\dfrac{1}{r_i}$. Thus, for example, take the equation

$$x^2 - 6x + 1 = 0,$$

of which the roots are

$$x = \cdot 171573 \text{ and } x = 5\cdot 828427.$$

To find successive approximations to the smaller root, take

$r_0 = 0,$ $r'_0 = 6 - \dfrac{1}{r'_1} = 5\cdot 8284,$

$r_1 = \dfrac{1}{6 - r_0} = \cdot 1667,$ $r'_1 = 6 - \dfrac{1}{r'_2} = 5\cdot 8284,$

$r_2 = \dfrac{1}{6 - r_1} = \cdot 1714,$ $r'_2 = 6 - \dfrac{1}{r'_3} = 5\cdot 8277,$

$r_3 = \dfrac{1}{6 - r_2} = \cdot 1716,$ $r'_3 = 6 - \dfrac{1}{r'_4} = 5\cdot 803,$

$r_4 = \dfrac{1}{6 - r_3} = \cdot 1716,$ $r'_4 = 6 - \dfrac{1}{r'_5} = 5\cdot 067,$

$r_5 = \dfrac{1}{6 - r_4} = \cdot 1716,$ $r'_5 = 6 - \dfrac{1}{r'_6} = 1\cdot 072,$

$r_6 = \dfrac{1}{6 - r_5} = \cdot 1716,$ $r'_6 = 6 - \dfrac{1}{r'_7} = \cdot 2029,$

$r_7 = \dfrac{1}{6 - r_6} = \cdot 1716,$ $r'_7 = 6 - \dfrac{1}{r'_8} = \cdot 1725,$

$r_8 = \dfrac{1}{6 - r_7} = \cdot 1716,$ $r'_8 = \cdot 1716.$

If the arithmetic at each step had been rigorous, we should have found $r'_7 = r_7$, $r'_6 = r_8$, and so on! Instead of coming back on the value 0 assumed for r_0, we find $r'_0 = 5\cdot 8284$, the greater root of the equation!

noticed (§§ 15–17). Remark now that as e (which is essentially positive in the actual problem) is increased from 0 to $+\infty$, each of the ratios R_1, R_2, ... R_i, R_{i+1}, ... increases from zero, each one more rapidly than the next in ascending order, until R_1 becomes $+\infty$, and suddenly changes to $-\infty$, and again goes on increasing till it again reaches $+\infty$ and suddenly $-\infty$, and so on. But before R_1 becomes ∞ the second time, R_2 becomes $+\infty$, $-\infty$, and again increases towards $+\infty$. The same holds for each of the other ratios; that is to say, as e increases continuously, each one of the ratios is always increasing except when its value reaches $+\infty$ and passes suddenly to $-\infty$. The order in which the values of the different ratios pass through ∞ is a subject of great interest and importance which requires careful examination. I hope to return to it, and meantime only remark that the formula (8) for calculating R_i from R_{i+1} shows:—

(1.) That no two consecutive ratios can be simultaneously negative.

(2.) While R_{i+1} increases from $-\infty$ to 0, R_i increases from 0 to a value somewhat (but very slightly) greater than $\dfrac{e}{(i+3)}$, and goes on increasing till it reaches ∞, when $R_{i+1} = \dfrac{2i+1}{2(i+3)}$.

(3.) When R_{i+1} is $> \dfrac{2i+1}{2(i+3)}$, and therefore when $R_{i+1} > 1$, R_i is negative.

From (1.) it follows that in the series of coefficients

$$K_2, \quad K_4, \quad K_6, \ldots$$

there cannot be two consecutive changes of sign. From (3.) it follows that each coefficient is less in absolute value than its predecessor if of the same sign, except when the predecessor is of opposite sign to the coefficient preceding *it*; and of two coefficients immediately following a change of sign, the second *may* be less than the first, but if so, only by a very small proportion of the value of either; but through nearly the whole range of values of e for which there is a change of sign from, say, K_i to K_{i+1}, K_{i+2} is $> K_{i+1}$ in absolute value. (For illustration of this see Laplace's series above, for his case of $e = 10$, for which he gives

$$K_2 = 1, \quad K_4 = 20\cdot1862, \quad K_6 = 10\cdot1164, \quad K_8 = -13\cdot1047,$$

$$K_{10} = -15\cdot4488, \quad K_{12} = -7\cdot4581, \quad K_{14} = -2\cdot1975 \ldots \&c.)$$

20. Laplace's brilliant invention which forms the subject of this article is capable of great extension, as I hope to show in a future communication. I have not hitherto found any trace

of it in treatises on differential equations; but I can scarcely think it probable that in some form or other it is not known to mathematicians who have occupied themselves with this subject. Known or unknown, it is of exceeding value and beauty as a purely mathematical method. As to Laplace's Dynamical Theory of Tides in general, I have much pleasure in concluding with a warmly appreciative statement by Airy which I find in his ' Tides and Waves,' art. (117).

" If, now, putting from our thoughts the details of the inves-
" tigation, we consider its general plan and objects, we must allow
" it to be one of the most splendid works of the greatest mathe-
" matician of the past age.   To appreciate this, the reader must
" consider, first, the boldness of the writer who, having a clear
" understanding of the gross imperfection of the methods of his
" predecessors, had also the courage deliberately to take up the
" problem on grounds fundamentally correct (however it might be
" limited by suppositions afterwards introduced); secondly, the
" general difficulty of treating the motions of fluids ; thirdly, the
" peculiar difficulty of treating the motions when the fluids cover
" an area which is not plane, but convex ; and, fourthly, the saga-
" city of perceiving that it was necessary to consider the earth as
" a revolving body, and the skill of correctly introducing this
" consideration.   The last point alone, in our opinion, gives a
" greater claim for reputation than the boasted explanation of
" the long inequality of Jupiter and Saturn."

Yacht ' Lalla Rookh,'
Frith of Lorn, August 11, 1875.

XXVI. *The 'Challenger's' Crucial Test of the Wind and Gravita-*
tion Theories of Oceanic Circulation.   By JAMES CROLL, *of*
H. M. Geological Survey[*].

NORTH *Atlantic*.—Perhaps the most remarkable result yet
obtained from the temperature-soundings of the ' Challen-
ger ' Expedition is the unexpected proof which they afford of the
physical impossibility, so far as the North Atlantic is concerned,
of any general interchange of equatorial and polar water due to
gravitation.

It is a condition absolutely essential to the gravitation theory
that the surface of the ocean should be highest in equatorial
regions and slope downwards to either pole.   Were water abso-
lutely frictionless, an incline, however small, would be sufficient

to produce a surface-flow from the equator to the poles; but to induce such an effect, some slope there must be, or gravitation could exercise no power in drawing the surface-water polewards.

The researches of the ' Challenger ' expedition bring to light the striking and important fact that the general surface of the North Atlantic, to be in equilibrium, must stand at a higher level than at the equator. In other words, the surface of the Atlantic is lowest at the equator, and rises with a gentle slope to well nigh the latitude of England. If this be the case, then it is mechanically impossible that, as far as the North Atlantic is concerned, there can be any such general movement as Dr. Carpenter believes. Gravitation can no more cause the surface-water of the Atlantic to flow towards the Arctic regions than it can compel the waters of the Gulf of Mexico up the Mississippi into the Missouri. The impossibility is equally great in both cases.

In order to prove what has been stated, let us take a section of the mid-Atlantic, north and south, across the equator ; and, to give the gravitation theory every advantage, let us select that particular section adopted by Dr. Carpenter as the one, of all others, most favourable to his theory, viz. section marked No. VIII. in his memoir lately read before the Royal Geographical Society *. The section referred to is shown in the accompanying diagram (p. 244). Its peculiarity, as will be observed, is the thinness of the warm strata at the equator as compared with those of the heated water in the North Atlantic. The fact that the polar cold water comes so near the surface at the equator is regarded by Dr. Carpenter as evidence in favour of the gravitation theory. On first looking at his section, it forcibly struck me that, if it was accurately drawn, the ocean, to be in equilibrium, would require to stand at a higher level in the North Atlantic than at the equator. In order, therefore, to determine whether this is the case or not, I asked the Hydrographer of the Admiralty to favour me with the temperature-soundings indicated in the section, a favour which was most obligingly granted. The following are the temperature-soundings at the three stations A, B, and C. The temperatures of C are the mean of six soundings taken along near the equator :—

* Proc. Roy. Geog. Soc. vol. xviii. p. 362.

Section of the Atlantic nearly North and South, between lat. 38 N. and lat. 38 S.
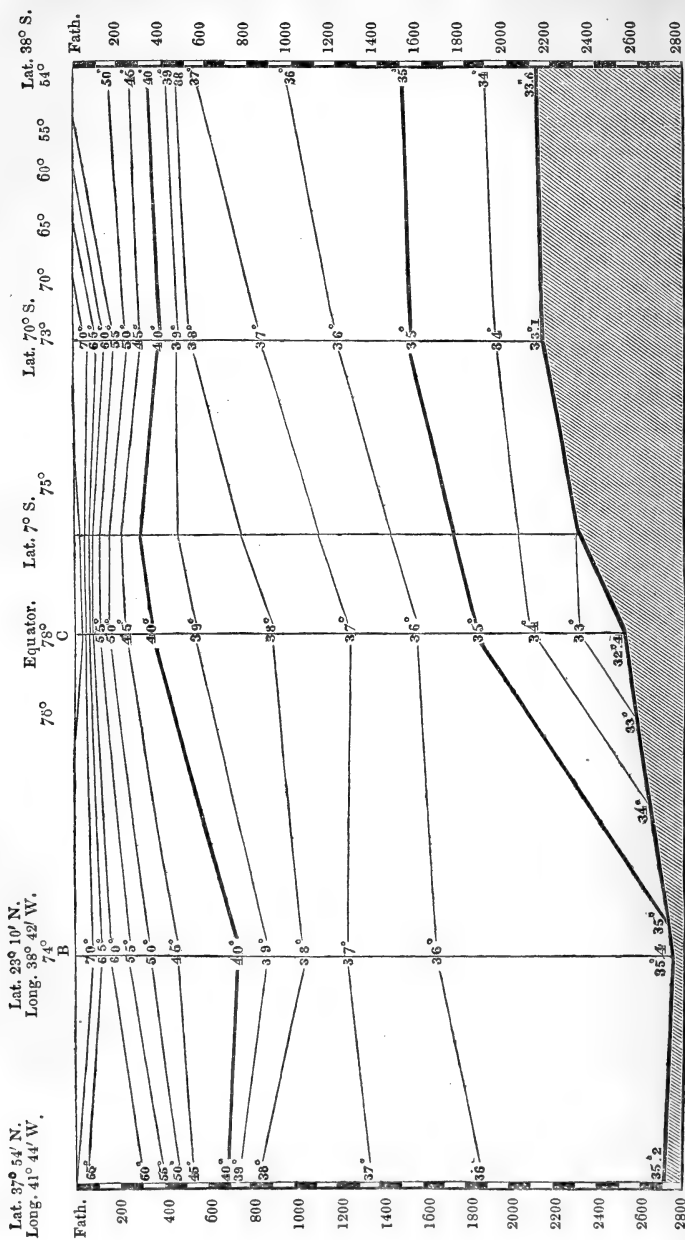
Depth in fathoms.	A. Lat. 37° 54′ N. Long. 41° 44′ W. Temperature.	B. Lat. 23° 10′ N. Long. 38° 42′ W. Temperature.	C. Mean of six temperature-soundings near equator.	
			Depth in fathoms.	Temperature.
Surface.	70·0	72·0	Surface.	77·9
100	63·5	67·0	10	77·2
200	60·6	57·6	20	77·1
300	60·0	52·5	30	76·9
400	54·8	47·7	40	71·7
500	46·7	43·7	50	64·0
600	41·6	41·7	60	60·4
700	40·6	40·6	70	59·4
800	38·1	39·4	80	58·0
900	37·8	39·2	90	58·0
1000	37·9	38·3	100	55·6
1100	37·1	38·0	150	51·0
1200	37·1	37·6	200	46·6
1300	37·2	36·7	300	42·2
1400	37·1	36·9	400	40·3
1500	......	36·7	500	38·9
2700	35·2	......	600	39·2
2720	......	35·4	700	39·0
			800	39·1
			900	38·2
			1000	36·9
			1100	37·6
			1200	36·7
			1300	35·8
			1400	36·4
			1500	36·1
			Bottom	34·7

On computing the extent to which the three columns A, B, and C are each expanded by heat, according to Muncke's Table of the expansion of sea-water for every degree Fahrenheit, I found that column B, in order to be in equilibrium with C (the equatorial column), would require to have its surface standing fully 2 feet 6 inches above the level of column C, and column A fully 3 feet 6 inches above that column. In short, it is evident that there must be a gradual rise from the equator to latitude 38° N. of 3½ feet. Any one can verify the accuracy of these results by making the necessary computations for himself; and he will find that, for columns of equal weight, column A is longer than column C by 3½ feet, and column B longer than column C by 2½ feet.

I may observe that had column C extended to the same depth as columns A and B, the difference of level would be considerably greater; for column C requires to balance only that portion

of columns A and B which lies above the equilibrium level of its base. And of course in computing the extent of the expansion of columns A and B, I have taken into account not the entire length of these columns, but only that portion which lies above that level. Suppose a depth of ocean equal to that of column C to extend to the North Pole and the polar water to have a uniform temperature of 33° from the surface to the bottom, then, in order to equilibrium, the surface of the ocean at the equator would require to be 4 feet 6 inches above that at the pole. But the surface of the ocean at B would be 7 feet, and at A 8 feet above the pole. Gravitation never could have caused the ocean to assume this form. It is impossible that this immense mass of warm water, extending to such a depth in the North Atlantic, can have been brought from equatorial regions by means of gravitation. And even if we suppose this accumulation of warm water can be accounted for by some other means, still its presence precludes the possibility of any such surface-flow as that advocated by Dr. Carpenter. For so long as the North Atlantic stands $3\frac{1}{2}$ feet above the level of the equator, gravitation can never move the equatorial waters polewards.

It will not do as an objection to assert that according to the gravitation theory the ocean never attains to a condition of static equilibrium. This is perfectly true, as I have shown on a former occasion[*]; but then it is the equator that is kept below and the poles above the level of equilibrium; consequently the disturbance of equilibrium between the equatorial and polar columns would actually tend to make the difference of level between the equator and the Atlantic greater than $3\frac{1}{2}$ feet, and not less, as the objection would imply.

There is another feature of this section irreconcilable with the gravitation theory. It will be observed that the accumulation of warm water is all in the North Atlantic, and that there is little or none in the south. But according to the gravitation theory, it ought to have been the reverse. For, owing to the unrestricted communication between the equatorial and antarctic regions, the general flow of water towards the south pole is, according to that theory, supposed to be greater than towards the north; and consequently the quantity of warm equatorial water in the South Atlantic ought also to be greater; Dr. Carpenter himself seems to be aware of this difficulty besetting the theory, and meets it by stating that "the upper stratum of the North Atlantic is not nearly as much cooled down by its limited polar underflow as that of the South Atlantic is by the vast movement of antarctic water which is constantly taking place towards the equator."

    * Phil. Mag. October 1871 ; 'Climate and Time,' Chapter ix.

But this "vast movement of antarctic water" necessarily implies a vast counter movement of warm surface-water; so that if there is more polar water in the South Atlantic to produce the cooling effect, there should likewise be more warm water to be cooled.

According to the wind theory of oceanic circulation the explanation of the whole is simple and obvious. It has already been shown that owing to the fact that the south-east trades are stronger than the north-east, and blow constantly over upon the northern hemisphere, the warm surface-water of the South Atlantic is drifted across the equator. It is then carried by the equatorial current into the Gulf of Mexico, and afterwards of course forms a part of the Gulf-stream.

The North Atlantic, on the other hand, not only does not lose its surface-heat like the equatorial and South Atlantic, but it receives from the Gulf-stream, in the form of warm water, an amount of heat, as we have seen, equal to one fourth of all the heat which it receives from the sun. The reason why the warm surface-strata are so much thicker in the North Atlantic than in the equatorial regions is perfectly obvious. The surface-water at the equator is swept into the Gulf of Mexico by the trade-winds and the equatorial current as rapidly as it is heated by the sun, so that it has not time to gather to any great depth. But all this warm water is carried by the Gulf-stream into the North Atlantic, where it accumulates. That this great depth of warm water in the North Atlantic, represented in the section, is derived from the Gulf-stream, and not from a direct flow from the equator due to gravitation, is further evident from the fact that temperature-sounding A in latitude 38° N. is made through that immense body of warm water, upwards of 300 fathoms thick, extending from Bermuda to near the Azores, discovered by the 'Challenger' expedition, and justly regarded by Captain Nares as an offshoot of the Gulf-stream. This, in Captain Nares's report, is No. 8 "temperature-sounding," between Bermuda and the Azores; sounding B is No. 6 "temperature-urve," between Teneriffe and St. Thomas.

There is an additional reason to the one already stated why the surface-temperature of the South Atlantic should be so much below that of the North. It is perfectly true that whatever amount of water is transferred from the southern hemisphere to the northern must be compensated by an equal amount from the northern to the southern hemisphere; nevertheless the warm water which is carried off the South Atlantic by the winds is not directly compensated by water from the north, but by that cold antarctic current whose existence is so well known to mariners from the immense masses of ice which it brings from the Southern Ocean.

North Pacific Ocean.—I have just had an opportunity of reading Dr. Carpenter's interesting " Summary of Recent Observations on the Temperature of the North Pacific made in the U.S.S. 'Tuscarora'"*. The temperature-soundings made in that vessel seem to me to show that the thermal condition of the North Pacific is just as irreconcilable with the gravitation theory as that of the North Atlantic. They show that the North Pacific is much colder than the North Atlantic, and that the immense stratum of warm water found in the latter is wanting in the North Pacific. But as the North Pacific is almost entirely cut off from the cold Arctic basin, its waters, according to the gravitation theory, instead of being colder, ought to be much warmer than those of the Atlantic.

But this is not all; these soundings bring to light the remarkable fact, as will be seen from the subjoined Table, that the North Pacific is actually warmer at latitude 52° than at latitude 43°.

Depth, in fathoms.	I. Lat. 43° 21′ N. Long. 149°12′ E.	II. Lat. 46° N. Long. 150° 45′ E.	III. Lat. 48° 40′ N. Long. 156° 7′ E.	IV. Lat. 51° 39′ N. Long. 164° 30′ E.	V. Lat. 52° 14′ N. Long. 173° 14′ E.
	Temperature.	Temperature.	Temperature.	Temperature.	Temperature.
Surface.	43·0	37·1	41·9	45·0	45·9
30	33·6	......	32·1	34·8	41·5
50	33·3	33·9	32·5	34·1	39·2
100	33·3	32·9	32·2	35·3	38·3
200	......	32·9	32·6	34·6	......
300	33·2	......	......	34·6	......
500	33·6	......	......	34·7	37·1

Now it is obvious that down to 500 fathoms (the depth to which these observations extend), for columns of equal weight, column V. (in lat. 52°) must be higher than column I. (in lat. 43°). It therefore follows, from the principles of hydrostatics, that unless the condition of things be totally the reverse below 500 fathoms, which is not at all likely, the top of column V. must stand at a higher level than that of column I.; and consequently if there be any surface-flow due to gravitation, it must be towards the equator, and not towards the pole.

Dr. Carpenter himself seems aware of the difficulties besetting the theory. But it appears to me that the explanation which he offers is as hostile to the theory as are the facts themselves to be explained. His explanation of the abnormally low tem-

* Proceedings of the Royal Geographical Society, vol. xix. No. 7, 1875.

perature of the North Pacific is that the surface-flow in that ocean is reversed, and that, instead of flowing, as everywhere else, from a lower to a higher latitude, it flows in the opposite direction.   His theory is that there is an underflow from the Antarctic regions northwards across the equator up to about latitude 52° N., and a counter surface-flow from that latitude southwards across the equator to the Antarctic regions.   And he gives a diagram illustrating this mode of circulation.   But it must be perfectly obvious that no such flow could be produced, according to the gravitation theory, from difference of temperature.   The difference of temperature, which is the prime mover according to this theory, results from the difference in the amount of heat received from the sun at equatorial and polar regions. The sea is warmer in equatorial than in temperate regions, because the amount of heat received from the sun is greater in the former than in the latter regions ; and for the same reason the sea in temperate is warmer than in polar regions.   It is therefore impossible, according to the gravitation theory, that the ocean at latitude 52° can be warmer than at the equator.   But, unless it were so, there could be no surface-flow due to gravitation from latitude 52° to the equator, because the surface of the ocean would not be higher in that latitude than at the equator, and without a slope gravitation could not act.   I, of course, admit that the ocean may actually be warmer in temperate than in polar regions ; for of this we have, as already shown, a good example in the case of the North Atlantic ; but then no such condition of things could arise from a circulation resulting from difference of temperature.   *A surface-flow from temperate regions across the equator, sustained by difference of temperature, is simply a mechanical impossibility.*

The truth is, that the adoption of such a mode of circulation makes the gravitation theory self-contradictory ; for, according to that theory, it is the lowness of the temperature of the ocean in high latitudes, in comparison with that in low, which is the reason why the surface-flow is toward the poles ; but in the case of the North Pacific this low temperature in high latitudes is adduced as a reason why the flow is in the reverse direction, viz. towards the equator.

According to the wind theory the explanation of the phenomena is plain and obvious.   The North Pacific is colder than the North Atlantic, because, although much larger, it nevertheless receives less heat from ocean-currents than the North Atlantic.   It is warmer at lat. 52° than at lat. 43°, because the former place lies more in the path of a warm current than the latter.

The Southern Ocean.—The thermal condition of the Southern

Ocean, as ascertained by the 'Challenger' expedition, appears to me to be also irreconcilable with the gravitation theory. Between the parallels of latitude 65° 42′ S. and 50° 1′ S., the ocean, with the exception of a thin stratum at the surface heated by the sun's rays, was found, down to the depth of about 200 fathoms, to be several degrees colder than the water underneath *. The cold upper stratum is evidently an antarctic current, and the warm underlying water an equatorial under-current. But, according to the gravitation theory, the colder water should be underneath.

The very fact of a mass of water 200 fathoms deep, and extending over 15 degrees of latitude, remaining above water of three or four degrees higher temperature shows how little influence difference of temperature has in producing motion. If it had the potency which some attribute to it, one would suppose that this cold stratum would sink down and displace the warm water underneath. If difference of density is sufficient to move the water horizontally, surely it must be more than sufficient to cause it to sink vertically.

It is difficult at present to estimate properly the importance of these results; for as oceanic circulation is the great agent in the distribution of heat over the globe, if it can be proved that this circulation is produced not by gravitation but by the system of prevailing winds, it is also proved that the thermal condition of the globe is dependent upon the prevailing winds, and that, consequently, any cause which will greatly affect the system of the winds will greatly affect the climatic condition of the globe. This necessary connexion between the system of the winds and the climatic condition of the globe leads us step by step towards the solution of the great problem of geological climate.

XXVII. *Cosmical Activity of Light.*   By PLINY EARLE CHASE, *Professor of Mathematics in Haverford College* †.

IN cosmical movements there are three prominent velocities, which may vary so as either to approach towards or to recede from equality :—

1. The velocity due to circular revolution at a given point, varying as $\sqrt{\dfrac{1}{r}}$. v_1.

2. The velocity of rotation for the same radius, or revolution retarded by internal work, varying as $\dfrac{1}{r}$. v_2.

3. The velocity of gravitating fall at the same point, $= \frac{v_1^2}{r}$, varying as $\frac{1}{r^2} \cdot v_3$.

The limiting velocity (v_0), towards which these three velocities all tend, is the velocity of light.

If the theory of Boscovich is true, or if matter is infinitely divisible, or if all the internal resistances, of heat-volume, mass-inertia, and every other kind could be dissipated, so as to allow an indefinite contraction of r, the limits of equality may be found either tangentially or radially.

When the tangential $v_1 = v_2$, we reach the limit between total aggregation and commencing dissociation (l_1). If further shrinkage takes place, the rotating particles gradually assume orbits of increasing excentricity.

When the radial velocity acquired by fall from an infinite distance, $\sqrt{2} \times v_1$, becomes equal to the mean velocity of radial oscillation synchronous with rotation $\left(\frac{2}{\pi} v_2\right)$, we reach the limit between total dissociation and commencing aggregation (l_2).

The upper limit (l_2) would be reached by all the subordinate planets before they had attained the lower limit (l_1) for the principal planets of their respective belts.

The upper limit would be reached by the principal planets, Earth and Jupiter, when they had attained the present limiting velocity of circular revolution at Sun's equator.

The limit of solar aggregation (l_1) is $\frac{1}{\pi}$ the velocity of light.

The limit of solar dissociation (l_2) is the velocity of light.

The time of revolution at $\frac{M}{2}$ (M being the solar *modulus* of light) is equivalent to the time of rotation for a sun expanded to Jupiter's centre of oscillation ($\frac{2}{3}$ of Jupiter's radius vector).

The ratio of retarded velocity in solar rotation $\left(\frac{v_1}{v_2}\right) = \frac{M}{2}$ $\div$ Jupiter's radius vector.

The limit of planetary dissociation (v_1 at Sun's equator) would carry a particle round the sun while a ray of light would traverse the linear pendulum of Sun's outermost planet ($\frac{2}{3}$ Neptune's radius vector).

The angular velocity of revolution at twice Neptune's distance = angular velocity of rotation due to a solar radius extending to Mercury's mean distance—a coincidence suggesting asteroidal or planetary masses, both beyond Neptune and below Mercury. Inasmuch as the linear velocity communicated by infinite fall to twice Neptune's distance equals the velocity of circular revolution at Neptune, this accordance seems to have fixed the prin-

cipal limits of the planetary belts*. Within those limits, planetary positions may be referred to simple circular pendulums, which are so jointly related that their harmonic vibrations tend to maintain the stability of the system.

The time of rotation for a given radius varying as the $\frac{4}{3}$ power of the time of revolution for same radius, the theoretical distance of each planet, in solar radii, may be found by multiplying the $\frac{4}{3}$ power of its number of pendulum units by the unit of distance, which is $\frac{9}{4}$ Sun's radius, Sun's surface being at a centre of explosive oscillation ($\frac{2}{3} \times \frac{2}{3} \times \frac{9}{4} = 1$). Symbolizing each pendulum by its planet's initial letters, we have the following synopsis of theoretical and observed distances .—

	Number of pendulum units.	(A) Theoretical mean distance.	(B) Actual mean distance.	$(A-B) \div (B)$.
Me	15	83·23	83·17	+·0007
Ve	24	155·76	155·42	+·0022
Ea	30	209·74	214·86	—·0239
Ma	42	328·48	327·38	+·0034
Ju	105	1114·75	1117·87	—·0028
Sa	168	2085·75	2049·51	+·0177
Ur	280	4121·54	4121·78	—·0001
Ne	392	6455·03	6453·06	+·0003

Column (A) *exactly* represents planetary positions—although, on account of orbital excentricities and joint perturbations, it gives *mean* positions only with a very close approximation.

If we take as our unit pendulum $(\frac{1}{2})^3$ of solar radius, the several planetary pendulums will represent the accordant radii of solar rotation, or the radii of solar expansion at which Sun's rotation would be synchronous with planetary revolution. The following comparison gives two theoretical values—(I.) in which the unit is $\frac{1}{8}$, (II.) in which the unit is increased by the ratio of Neptune's mean excentricity, 1 per cent. :—

	No. of pendulum units.	I. Theoretical.	II. Theoretical.	Actual.	I. Error.	II. Error.
Me	15	1·875	1·894	1·892	—·0272	—·0176
Ve	24	3·000	3·030	3·023	—·0090	+·0001
Ea	30	3·750	3·787	3·855	—·0074	+·0023
Ma	42	5·250	5·302	5·287	—·0070	+·0028
Ju	105	13·125	13·256	13·279	—·0116	—·0017
Sa	168	21·000	21·210	20·923	+·0037	+·0137
Ur	280	35·000	35·350	35·334	—·0095	+·0005
Ne	392	49·000	49·490	49·454	—·0092	+·0007

* My harmonic anticipation of planetary or asteroidal matter at a distance from the sun equivalent to ·269 of Earth's mean radius vector, which was so promptly confirmed by the sun-spot observations of De La Rue, Stewart, and Loewy, is still further corroborated by the recent papers of Leverrier.

The I. theoretical column would imply a solar rotation in 25·02 days, which is very nearly the mean time of rotation of the sun-spots; column II. gives 24·52 days—the assumed actual value being Spörer's estimate for the solar equator, 24·57 days.

The pendulum orbits may be referred to extremities, or to centres of linear oscillation of primitive pendulums, as follows :—

With a solar *r* extending to Sa's centre of oscillation, *r*, Sa, Ur, Ne would represent the two extremities and the centres of oscillation of a linear pendulum.

Sun is at the centre of oscillation of one division of a pendulum, of which Ma, Ju, Sa represent the centres of oscillation and an extremity.

Sun is at a centre of oscillation between Ea and Me.

Ea is at a centre of oscillation between Ma and Ve.

Ve is at a centre of oscillation between Ma and Me.

The free extremity of the second pendulum is at a centre of oscillation between Ve and Me.

Each of the divisions of the first pendulum is equivalent to the corresponding solar diameter; and the pendulum-orbit is symmetrically divided in all directions from the centre.

Combining these several results with the luminous and electrical accordances which have been discovered by Weber and Kohlrausch, Thomson, Maxwell, and Edlund, and with the chemical and other physical harmonics which I have pointed out elsewhere, we may find a profound scientific truth in the doctrine that the first act of creation was the divine command, " Let there be light."

According to some statements of the nebular hypothesis, if the Sun (or a planet) were expanded until its surface should reach each of the planets (or satellites) in succession, its time of rotation in each position would coincide with the time of revolution of the corresponding planet (or satellite). This would be true of an atmosphere extending to the point where the equatorial, centripetal, and centrifugal forces are equal; but is that assumption justifiable? and does it lend any sanction to the nebular rather than to the meteoric hypothesis?

Another common fallacy is the assumption that the ultimate absorption of our system in the Sun would result in stagnation and death. In our present ignorance of the constitution of matter such a belief is unwarrantable. It is quite possible that the equatorial velocity of rotation will ultimately surpass the velocity of revolution. The mass might thus be dissipated, as a condition precedent to renewed life and the formation of new systems.

Philadelphia, Pa., July 1875.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON A PROPERTY OF AN ELECTRIZED WATER-SURFACE.
BY G. LIPMANN.

A MASS of water contained in a glass vessel is put in communication with the earth by means of a platinum wire. If a stick of resin be rubbed and then brought near the water, positive electricity is drawn from the earth and distributed at the surface of the water. The platinum wire, which serves as electrode of entry, has a flow of positive electricity; at it bubbles of oxygen gas are liberated, in quantity proportional to the quantity of electricity entering: at least this takes place if an electrode of very small surface is employed, a Wollaston point. The fact of the liberation of oxygen under these circumstances is well known; it has been verified, notably by M. Buff and by M. Soret.

After oxygen has been liberated, the hydrogen which was combined with it remains in excess in the mass of water or else at its surface. This excess, proportional to the charge, remains in some way *dissembled* as long as the water is electrized; but at the instant of the discharge the hydrogen is disengaged.

The removal of the stick of resin suffices. The charge, which was retained by influence, passes away into the earth through the platinum point. This point serving as electrode of exit has a flow of positive electricity; it liberates bubbles of hydrogen gas. The *dissembled* hydrogen is therefore found again during the discharge; the whole of it is recovered.

In fact, according to Faraday's law, the same quantity of electricity which liberates 1 equivalent of oxygen when it enters, sets free precisely 1 equivalent of hydrogen at its departure.

Since the whole of the *dissembled* hydrogen is to be recovered, no part of it can be taken away by diffusion, or oxidation, or any physical or chemical action that leaves the electric charge intact. In other terms, the concealed hydrogen is neither combined nor dissolved; and yet it is really there, since we can cause its liberation by removing the stick of resin.

Moreover the words *dissolved* or *combined* might be applied to hydrogen retained in the interior of a mass; but here, it seems, we have a first instance of another order of material union. The dissembled hydrogen is all held at the *surface* of the water—I mean, in that portion of the body in which the electric charge is distributed.

In fact, any portion whatever of the interior mass of water may be replaced by air; and as long as the surface is unchanged, the charge of electricity and consequently the quantity of dissembled hydrogen are unchanged also. Thus the mass can be hollowed out without changing the quantity of the hydrogen; this is therefore found again at the surface.

In the same way a mass of water electrized unequally, contains at its surface an excess of hydrogen proportionate to the electric charge.—*Comptes Rendus de l'Acad. des Sciences*, August 9, 1875, vol. lxxi. pp. 280, 281.

ON THE INFLUENCE OF THE TEXTURE OF IRON UPON ITS MAGNETISM. BY L. KÜLP, OF DARMSTADT.

The following is a statement of two series of experiments which I made upon two iron bars of unlike texture, 48 millims. in length, 11 millims. in thickness, and each weighing 34·2 grammes.

Bar I. had a granular, while bar II. had a decidedly fibrous structure—recognized in both bars from the fracture-surfaces. For the observations the bars were placed in an induction-coil of 1600 turns; and the moments were read off in the well-known manner on a Wiedemann mirror-compass distant 500 centims., by means of a telescope with scale at 4 metres distance.

In the following Tables the first columns contain the relative intensities of the current through the coil, and the second contain the deflections (in scale-divisions) corresponding to the respective moments.

TABLE *a.*—Bar I.

Relative intensity of current.	Deflection.
0·0874	3·5
0·1727	8·5
0·2962	16
0·4142	23·5
0·5984	34
0·7399	43
0·9260	56
1·0761	65
1·6642	103

TABLE *b.*—Bar II.

Relative intensity of current.	Deflection.
0·0722	4
0·1907	10
0·3482	19
0·5317	29
0·7080	41·5
0·9067	55
1·0913	65
1·6642	104

From this we get the remarkable result that, for equal intensities of the current, the two bars gave almost exactly the same induced magnetic moments, and consequently that the very different structure of the two pieces of iron employed had no influence whatever upon the amount of the induced magnetic moment.—Poggendorff's *Annalen*, 1875, vol. clv. pp. 320, 321.

ON MAGNETS FORMED FROM COMPRESSED POWDERS. BY J. JAMIN.

In 1836, in the *Mémoires de l'Académie* of Stanislas, De Haldat published an interesting observation : he had put iron filings into a brass tube closed by two screw stoppers; he magnetized it by the usual process, and ascertained that it had acquired, and retained, at its extremities two opposite poles. The polarity was not sensibly augmented when the stoppers were tightened, and diminished but slowly when increasing quantities of river-sand were mixed with the filings. In all cases the polarity remained very feeble; and it vanished when the metallic grains were displaced by agitating the tube.

This observation is accurate but imperfect. I have repeated it by forcibly compressing with a small hydraulic press the iron filings in the tube. When they begin to aggregate, the polarity is seen to augment considerably, and continues to increase with the pressure. I exhibit to the Academy tubes of from 8 to 10 centims. length and 3 centims. diameter, which attract at least as much steel filings as would be attracted by pieces of good steel of the same dimensions.

As the filings I employed were of unknown origin, I had some prepared in my presence from very soft iron, perfectly reduced and without appreciable coercive force. The results were quite equal to the former. Here, then, is a metal which has no coercive force when it is continuous, but acquires a coercive force equal to that of steel when we reduce it to minute fragments and then make them contiguous by pressure. Must not the polarity observed be attributed to this discontinuity? and is not the coercive force of steel to be explained by the same cause?

The distribution of the force in a magnet cannot be accounted for without regarding it as composed of rows of minute magnetic elements with opposite poles reacting on each other at a distance; and it is verified that the quantities of separate magnetism in each of them are increased by this reaction, from the extremity to the middle line (Lamé, *Physique*, vol. iii. p. 100). Up to the present it seems to be assumed that these elements are the molecules themselves; the preceding experiment seems to show they are formed either by fragments of iron in proximity to one another, or by minute crystals agglomerated as in steel.

When, before pressing the filings, we interpolate among them substances which render the mass more homogeneous, the same polarity cannot be given as when they are unmixed. For example, by making a paste of chloride of iron and the filings and pressing it, we obtain, after a few days, a subchloride continuous in appearance, capable of being filed and polished like pure iron, but hardly magnetizable.

Iron reduced by hydrogen, and scales of oxide of iron, behave like iron filings; but magnetic or diamagnetic substances mixed with the filings alter notably the faculty they possess of being magnetized. The study of all these circumstances promises interesting researches. Hitherto I have had at my disposal only insufficient apparatus and a small laboratory press. It is probable that by greatly increasing the compression of the powders the coercive force will be found to increase to a maximum, and afterwards diminish when the approximation of the fragments has restored a sufficient degree of continuity to the mass. I shall soon be in a position to communicate to the Academy the result of these new researches.—*Comptes Rendus de l'Académie des Sciences*, Aug. 2, 1875, vol. lxxxi. pp. 205–207.

XXIX. *Studies on Magnetic Distribution.* *By* Henry A. Row-land, *of the Johns Hopkins University, Baltimore, Md., U.S.A.*[*]

Part I.—*Linear Distribution.*

Contents.

I. Preliminary remarks.
II. Mathematical theory.
III. Experimental methods for measuring linear distribution.
IV. Iron rods magnetized by induction.
V. Straight electromagnets and permanent steel magnets.
VI. Miscellaneous applications.

I.

IN a paper of mine published about two years ago, I alluded to some investigations which I had made in 1870 and 1871 on the distribution of magnetism. It is with diffidence that I approach this subject, being aware of the great mathematical difficulties with which it is surrounded. But as the facts are still in advance of what is known on the subject, and as I see that other investigators[†] are following hard upon my footsteps, I thought it would be well to publish them, particularly as it is no fault of mine that they did not appear some years ago[‡]. The mathematical theory which I give, although not particularly elegant, will at least be found to present the matter in a new and more simple light, and may be considered simply as a deve-

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[*] Communicated by the Author.
[†] Particularly M. Jamin.
[‡] All the experiments referred to in this paper were made in the winter of 1870–71.

lopment of Faraday's idea of the analogy between a magnet and a voltaic battery immersed in water.   I shall throughout speak of the conduction of, and resistance to, lines of magnetic force, and shall otherwise treat them as similar to lines of conducted electricity or heat, it now being well established from the researches of Professor Maxwell and others that this method gives exactly the same results as the other method of considering the action to take place at a distance.

In arranging this paper I have thought best to give the theory of the distribution first, and then afterwards to see how the results agree with experiment; in this way we can find out the defects of the theory, and what changes should be made in it to adapt it to experiment.

At present I am acquainted with two formulæ giving the distribution of magnetism on bar magnets: the first was given by Biot, in his *Traité de Physique Expérimentale et Mathématique*, vol. iii. p. 77, and was obtained by him from the analogy of the magnet to a dry electric pile, or to a crystal of tourmaline electrified by heat.   He compared his formula with Coulomb's observations, and showed it to represent the distribution with considerable accuracy.   Green, in his 'Essay,' has obtained a formula which gives the same distribution; but he obtains it by a series of mathematical approximations which it is almost impossible to interpret physically.   M. Jamin has recently used a formula of the same form; but I have as yet been unable to find how he obtained it.   My own formulæ are also quite similar to these, but have the advantage of being obtained in a more simple manner than Green's; and, what is of more consequence, all the limitations are made at once, after which the solution is exact; so that although they are only approximate, yet we know just where they should differ from experiment.

II.

If we take an iron bar and magnetize one end of it either by a magnet or helix, we cause lines of magnetic induction[*] to enter that end of the bar, and, after passing down it to a certain distance, to pass out into the air and so round to the bar again to complete their circuit.   At every part of their circuit they encounter some resistance, and always tend to pass in that direction where it is the least: throughout their whole course they obey a law similar to Ohm's law; and the number of lines passing in any direction between two points is equal to the difference

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[*] For difference between lines of magnetic force and lines of magnetic induction see Maxwell's 'Treatise on Electricity and Magnetism,' arts. 400, 592, and 604.

of magnetic potential of those points divided by the resistance to the lines.

The complete solution of the problem before us being impossible, let us limit it by two hypotheses. First, let us assume that the permeability of the bar is a constant quantity; and secondly, that the resistance to the lines of induction is composed of two parts, the first being that of the bar, and the second that of escaping from the bar into the medium—and that the latter is the same at every part of the bar. The first of these assumptions is the one usually made in the mathematical theory of magnetic induction; but, as has been shown by the experiments of Müller, and more recently by those of Dr. Stoletow and myself, this is not true; and we shall see this when we come to compare the formula with experiment. The second assumption is more exact than the first for all portions of the bar except the ends.

Let us first take the case of a rod of iron with a short helix placed on any portion of it, through which a current of electricity is sent. The lines of magnetic induction stream down the bar on either side: at every point of the bar two paths are open to them, either to pass further down the rod, or to pass out into the air. We can then apply the ordinary equations for a derived circuit in electricity to this case.

Let μ be the magnetic permeability of the iron,

R be the resistance of unit of length of the rod,

R' be the resistance of medium along unit of length of rod,

ρ be the resistance at a given point to passing down the rod,

s be the resistance at the end of the rod,

Q'^{*} be the number of lines of induction passing along the rod at a given point,

$Q'_\epsilon {*}{\dagger}$ be the number of lines of induction passing from the rod into the medium along a small length of the rod ΔL,

L be the distance from the end of the rod to a given point,

$$r = \sqrt{\frac{R}{R'}},$$

$$A = \frac{\sqrt{RR'} + s}{\sqrt{RR'} - s}.$$

To find ρ, the ordinary equation for the resistance of a derived circuit gives

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* These are the surface-integrals of magnetic induction (See Maxwell's 'Electricity,' art. 402)—the first across the section of the bar, and the second along a length ΔL of the surface of the bar.

† It is to be noted that Q'_ϵ, when ΔL is constant, is nearly proportional to the so-called surface-density of magnetism at the given point.

$$\rho + d\rho = \frac{(\rho + \mathrm{R}d\mathrm{L})\dfrac{\mathrm{R}'}{d\mathrm{L}}}{\rho + \mathrm{R}d\mathrm{L} + \dfrac{\mathrm{R}'}{d\mathrm{L}}},$$

whence

$$\frac{d\rho}{d\mathrm{L}} = \frac{1}{\mathrm{R}'}\left(\mathrm{R}\mathrm{R}' - \rho^2\right),$$

and

$$\rho = \sqrt{\mathrm{R}\mathrm{R}'}\frac{\mathrm{A}\epsilon^{2r\mathrm{L}} - 1}{\mathrm{A}\epsilon^{2r\mathrm{L}} + 1}. \quad \cdots \quad (1)$$

To find Q', we have

$$d\mathrm{Q}' = \frac{\mathrm{Q}'\rho}{\mathrm{R}'}\,d\mathrm{L},$$

whence

$$\mathrm{Q}' = \frac{\mathrm{C}}{\mathrm{A}+1}\left(\mathrm{A}\epsilon^{r\mathrm{L}} + \epsilon^{-r\mathrm{L}}\right)*, \quad \cdots \quad (2)$$

and

$$\mathrm{Q}'_\epsilon = \frac{\mathrm{Q}'\rho\Delta\mathrm{L}}{\mathrm{R}'} = r\frac{\mathrm{C}\Delta\mathrm{L}}{\mathrm{A}+1}\left(\mathrm{A}\epsilon^{r\mathrm{L}} - \epsilon^{-r\mathrm{L}}\right). \quad \cdots \quad (3)$$

When L is very large, or $s = \sqrt{\mathrm{R}\mathrm{R}'}$, we have

$$\mathrm{Q}' = \mathrm{C}_{l}\epsilon^{r\mathrm{L}'} \text{ and } \mathrm{Q}'_\epsilon = \mathrm{C}_{l}r\Delta\mathrm{L}\epsilon^{r\mathrm{L}_{l}},$$

in which L_{l} is reckoned from an origin at any point of the rod.

These equations give the distribution on the part outside the helix; and we have now to consider the part covered by the helix. Let us limit ourselves to the case where the helix is long and thin, so that the field in its interior is nearly uniform.

Fig. 1.
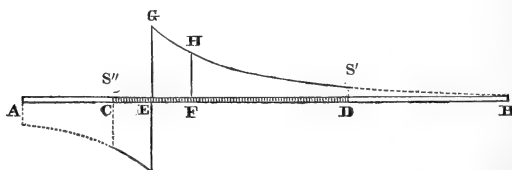
As we pass along the helix, the change of magnetic potential due to the helix is equal to the product of the intensity of the field multiplied by the distance passed over; so that in passing over an elementary distance dy the difference of potential will be $\mathfrak{H}dy$. The number of lines of force which this difference of po-

* This could have been obtained directly from the equation $\dfrac{d^2\mathrm{Q}'}{d\mathrm{L}^2} = \mathrm{Q}'r^2$,

and Q'_ϵ from the equation $\mathrm{Q}'_\epsilon = \dfrac{d\mathrm{Q}'}{d\mathrm{L}}\Delta\mathrm{L}$.

tential causes in the rod will be equal to $\mathfrak{H}dy$ divided by the sum of the resistances of the rod in both directions from the given point. These lines of force stream down the rod on either side of the point, creating everywhere a magnetic potential which can be calculated by equation (2), and which is represented by the curves in fig. 1. In that figure A B is the rod, C D the helix, and E the element of length dy. Now, if we take all the elements of the rod in the same way and consider the effect at H F, the total magnetic potential at this point will, by hypothesis No. 1, be equal to the sum of the potentials due to all the elements dy.

Let $\Delta Q'$ be the number of lines of force produced in the bar at the point E due to the elementary difference of potential at that point, $\mathfrak{H}dy$,

$\Delta Q''$ be the number of lines of force arriving at the point F due to the same element,

Q''_ϵ be the number of lines passing from bar along length ΔL,

ρ_y be the sum of the resistances of the bar in both directions from E,

ρ_x be resistance at F in direction of D,

y be the distance D E,

x be the distance D F,

b be the distance C D,

s'' and s' be the resistance of the bar &c. respectively at C in the direction of A, and at D in direction of B,

$\mathfrak{H}$ be the magnetizing-force of helix in its interior.

Let

$$A' = \frac{\sqrt{RR'} + s'}{\sqrt{RR'} - s'}, \quad A'' = \frac{\sqrt{RR'} + s''}{\sqrt{RR'} - s''}, \quad r = \sqrt{\frac{R}{R'}};$$

$$\rho_y = \frac{2(A'A''\epsilon^{2rb} - 1)}{(A''\epsilon^{2ry} + 1)(A'\epsilon^{2r(b-y)} + 1)} \sqrt{RR'},$$

$$\Delta Q' = \frac{\mathfrak{H}dy}{\rho_y},$$

$$\Delta Q'' = \Delta Q'(A''\epsilon^{rx} + \epsilon^{-rx}) \frac{\epsilon^{ry}}{A''\epsilon^{2ry} + 1}$$

$$= \frac{\mathfrak{H}}{2\sqrt{RR'}} \frac{A''\epsilon^{rx} + \epsilon^{-rx}}{A'A''\epsilon^{2rb} - 1} (A'\epsilon^{2r(b-y)} + 1)\epsilon^{ry}dy,$$

$$Q''_\epsilon = \frac{\Delta L}{R'} \int \Delta Q'' \rho_x = \frac{\mathfrak{H}\Delta L}{2R'} \frac{A''\epsilon^{rx} - \epsilon^{-rx}}{A'A''\epsilon^{2rb} - 1} \int_x^b (A'\epsilon^{2rb}\epsilon^{-ry} + \epsilon^{ry})dy$$

$$= \frac{\mathfrak{H}\Delta L}{2R'r} \frac{A''\epsilon^{rx} - \epsilon^{-rx}}{A'A''\epsilon^{2rb} - 1} \left[A'\epsilon^{2rb}(\epsilon^{-rx} - \epsilon^{-rb}) + \epsilon^{rb} - \epsilon^{rx} \right]. \quad (4)$$

This gives the positive part of Q''_ϵ.   To find the negative part, change x into $b-x$, A' into A'', and A'' into A', and then change the sign of the whole.

When the helix is symmetrically placed on the bar, we have $s'=s''$, $A'=A''$; whence, adding the positive and negative parts together, we have

$$Q''_\epsilon = \frac{\mathfrak{H}\Delta L}{2\sqrt{RR'}} \frac{1-A'}{A'\epsilon^{rb}-1} (\epsilon^{r(b-x)} - \epsilon^{rx}), \quad . \quad . \quad . \quad (5)$$

which gives the number of lines of induction passing out from the rod along the length ΔL when the helix is symmetrically placed on the rod.

To get the number of lines of induction passing along the rod at a given point, we have

$$Q'' = \int_0^x Q''_\epsilon dx = \frac{\mathfrak{H}}{2R} \frac{1-A'}{A'\epsilon^{rb}-1} (\epsilon^{rb}+1-\epsilon^{rx}-\epsilon^{r(b-x)}+C''', \quad (6)$$

where

$$C''' = -\frac{\mathfrak{H}}{r} \frac{\epsilon^{rb}-1}{(A'\epsilon^{rb}-1)(\sqrt{RR'}-s')}.$$

When the bar extends a distance L' out of both ends of the helix, so that

$$s' = \sqrt{RR'} \frac{\epsilon^{2rL'}+1}{\epsilon^{2rL'}-1} \text{ and } A' = -\epsilon^{2rL'},$$

we have

$$C''' = -\frac{\mathfrak{H}}{r} \frac{(\epsilon^{rb}-1)(\epsilon^{2rL'}-1)}{(\epsilon^{rb}\epsilon^{2rL'}+1)2\sqrt{RR'}}.$$

It may be well, before proceeding, to define what is meant by magnetic resistance, and the units in which it is measured.   If μ is the magnetic permeability of the rod, we can get an idea of the meaning of magnetic resistance in the following manner. Suppose we have a rod infinitely long placed in a magnetic field of intensity $\mathfrak{H}$ parallel to the lines of force.   Let Q' be the number of lines of inductive force passing through the rod, or the surface-integral of the magnetic induction across its section; also let a be the area of the rod.   Then by definition $\mu = \dfrac{Q'}{a\mathfrak{H}}$.   If L is the length of the rod, the difference of potential at the ends will be $L\mathfrak{H}$; hence

$$Q' = \frac{L\mathfrak{H}}{R_l}, \text{ and } R_l = \frac{L\mathfrak{H}}{Q'} = \frac{L}{a\mu},$$

and R in the formulæ becomes

$$R = \frac{R_l}{L} = \frac{1}{a\mu}.$$

It is almost impossible to estimate R' theoretically, seeing that it will vary with the circumstances. We can get some idea of its nature, however, by considering that the principal part of it is due to the cylindric envelope of medium immediately surrounding the rod. The resistance of such an envelope per unit of length of rod is

$$\frac{1}{2\pi\mu_l} \text{hyp. log} \frac{D}{d},$$

where D is the diameter of the envelope, d of the rod, and μ' the permeability of the medium. But we are not able to estimate D. If, however, we have two magnetic systems similar in all their parts, it is evident that beyond a certain point similarly situated in each system we may neglect the resistance of the medium, and $\frac{D}{d}$ will be the same for the two systems. Hence R' is approximately constant for rods of all diameters in the same medium, and r takes the form

$$r = \frac{2}{d}\sqrt{\frac{1}{\pi\mu R'}}. \quad . \quad . \quad . \quad . \quad . \quad (7)$$

It is evident that the reasoning would apply to rods of any section as well as circular.

In Green's splendid essay (Reprint, p. 111, or Maxwell's 'Treatise on Electricity and Magnetism,' art. 439) we find a formula similar to equation (5), but obtained in an entirely different manner, and applying only to rods not extending beyond the helix. In the ' Reprint,' β corresponds to my r; and its value, using my notation, is obtained from the equation

$$\cdot 231863 - 2 \text{ hyp. log} p + 2p = \frac{4}{(\mu-1)p^2}, \quad . \quad . \quad (8)$$

where $p = \frac{rd}{2}$.

If we make μ a constant in this formula, we must have $p = \frac{rd}{2} = \text{constant}$; hence

$$r \propto \frac{1}{d},$$

which is the same result for this case as from equation (7).

When μ in the two formulæ is made to vary, the results are not exactly the same; but still they give approximately the same results for the cases we shall consider; and since the formula is at the best only approximate, we shall not spend time in discussing the merits of the two.

III.

Among the various methods of measuring linear magnetic distribution, we find few up to the present time that are satisfactory. Coulomb used the method of counting the number of vibrations made by a magnetic needle when near various points of the magnet. Thus, in the curve of distribution most often reproduced from his work, he used a magnetized steel bar 27 French inches long and 2 lines in diameter placed vertically; opposite to it, and at a distance of 8 lines, he hung a magnetic needle 3 lines in diameter and 6 lines long, tempered very hard; and the number of oscillations made by it was determined. The square of this number is proportional to the magnetic field at that point, supposing the magnetism of the needle to be unchanged; and this, corrected for the magnetism of the earth, gives the magnetic field due to the magnet alone. This for points near the magnet and distant from the ends is nearly proportional to the so-called magnetic surface-density opposite the point. At the end Coulomb doubled the quantity thus found, seeing that the bar extended only on one side of the needle.

It will be seen that this method is only approximate, and almost incapable of giving results in absolute measure. The effect on the needle depends not only on that part of the bar opposite the needle, but on portions to either side, and gives, as it were, the average value for some distance; in the next place, the correction at the end, by multiplying by 2, seems to be inadequate, and gives too small a result compared with other parts. For at points distant from the end the average surface-density at any point will be nearly equal to the average for a short distance on both sides, while at the end it will be greater than the average of a short distance measured back from the end. To these errors must be added those due to the mutual induction of the two magnets.

The next method we come to is that which has been recently used by M. Jamin, and consists in measuring the attraction of a piece of soft iron applied at different points of the magnet. In this case it does not seem to have been considered that the attraction depends not only on the magnetic density at the given point, but also on that around it, and that a piece of soft iron applied to a magnet changes the distribution immediately at all points, but especially at that where the iron is applied. The change is of course less when the magnet is of very hard steel and the piece of soft iron small. Where, however, we wish to get the distribution on soft iron, it becomes a quite serious difficulty. Another source of error arises from the fact that the coefficient of magnetization of soft iron is a func-

tion of the magnetization : this source of error is greatest when the contact-piece is long and thin, and is a minimum when it is short and thick and not in contact with the magnet.   Hence this method will give the best results when the contact-piece is small and in the shape of a sphere and not in contact with the magnet, and when the method is applied to steel magnets.   But after taking all these precautions, the question next arises as to how to obtain the magnetic surface-density from the experiments. Theory indicates, and M. Jamin has assumed, that the attractive force is nearly proportional to the square of the surface-density.   But experiment does not seem to confirm this, except where there is some distance between the two bodies, at least in the case of a sphere and a plane surface, as in Tyndall's experiments (Phil. Mag. April 1851).   It is not necessary at present to consider the cause of this *apparent* discrepancy between theory and experiment ; suffice it to say that the explanation of the phenomenon is without doubt to be sought for in the variable character of the magnetizing-function of iron.   All I wish to show is that the attraction of iron to a magnet, especially when the two are in contact, is a very complicated phenomenon, whose laws in general are unknown, and hence is entirely unsuitable for experiments on magnetic distribution.

A third method is that used in determining the correction for the distribution on the magnets in finding the intensity of the earth's magnetism.   Usually the distribution is not explicitly found in this case ; but it is easy to see how it might be.   Thus, one way would be as follows :—Take the origin of coordinates at the centre of the magnet.   Develop the distribution in an ascending series of powers of x with unknown constant coefficients.   Calculate the magnetic force due to this distribution for any points along the axis, or else on a line perpendicular to the magnet at its centre.   Determine the force at a series of points extending through as great a range and as near the magnet as possible.   These experiments give a series of equations from which the coefficients in the expansion can be determined. Other and better methods of expansion might be found, except for short magnets, where the method suggested is very good.

The similarity of this method to that used by Gauss in determining the distribution on the earth is apparent.

A fourth method is similar to the above, except that the lines of force around the magnet are measured and calculated instead of the force.

The last two methods are very exact, but are also very laborious, and therefore only adapted to special investigations. Thus, by the change in direction of the lines of force around the magnet, we have a delicate means of showing the change in dis-

tribution, as, for instance, when the current around an electro-magnet varies.

The fifth method is that used lately in some experiments of Mr. Sears (American Journal of Science, July 1874), but only adapted to temporary magnetization. At a given point on the bar a small coil of wire is placed, and the current induced in it measured by the swing of the galvanometer-needle when the bar is demagnetized. It does not seem to have been noticed that what we ordinarily consider the magnetic distribution is not directly measured in this way; and indeed, to get correct results, the magnetization should have been reversed, seeing that a *large portion* of the magnetization will not disappear, on taking away the magnetizing-force, where the bar is long. The quantity which is directly measured is the surface-integral of the temporary magnetic induction *across the section* of the bar, while the magnetic surface-density is proportional to the surface-integral of magnetic induction *along a given portion of the bar*. In other words, the quantity measured is Q instead of $\frac{Q_\epsilon}{\Delta L}$. We can, however, derive one from the other very easily.

The sixth and last method is that which I used first in 1870, and by which most of my experiments have been performed. This consists in sliding a small coil of wire, *which just fits the bar and is also very narrow,* along the bar inch by inch, and noting the induced current over each inch by the deflection of a galvanometer-needle. This measures Q_ϵ, except for some corrections which I now wish to note. In the first case, to give exact results, the lines of force should pass out perpendicular to the bar, or the coil must be very small. But even when the last condition is fulfilled errors will be introduced at certain portions of the bar. The error is vanishingly small in most cases, except near the ends; and even there it is not large, except in special cases; for at this part the lines of force pass forward toward the end of the bar, and so the observation next to the end may be too small, while that at the end is too large. The correction can be made by finding where the lines of force through the centre of the section of the coil in its two positions meet the bar. The error from this source is not large, and may be avoided to a great extent.

One very great advantage in the method of induced currents is the facility with which the results can be reduced to absolute measure by including an earth-inductor in the circuit as I have before described (Phil. Mag. August 1873). There is also no reaction (except a temporary one) between the magnet and current; so that the distribution remains unchanged. Hence it

seems to me that this method is the only one capable of giving exact results directly.

The coils of wire which I used consisted of from twenty to one hundred turns of fine wire wound on thin paper tubes which just fitted the bar and extended considerably beyond the coils. The coils were mostly from ·1 to ·25 of an inch wide and from ·1 to ·2 inch thick. A measure being laid by the side of the given bar under experiment, the coil was moved from one division of the rule to the next very quickly, and the deflection produced on an ordinary astatic galvanometer noted. After experience this could be done with great accuracy. It might be better in some cases to have the coil slide over a limited distance on the tube, though for the use to which I intend to put the results the other is best.

Up to $35°$ Q_ϵ is nearly proportional to the deflection; and when any larger value is put down in the Tables, it is the sum of two or more deflections. I have not the data in most cases to reduce my results to absolute measure, but took pains to ensure that certain series of experiments should be comparable among themselves.

Having measured Q_ϵ at all points of a rod, we may find Q by adding up the values of Q_ϵ from the end of the rod.

The magnetizing force to which the bar was subjected was in all cases a helix placed at some part of the bar. The iron bars were of course demagnetized thoroughly before use by placing them in the proper position with reference to the magnetic meridian and striking them.

In the Tables L is the distance in inches from the zero-point, Q_ϵ is the deflection of the galvanometer when the helix is passed between the points indicated in the first column. Thus, in Table II., $34·7$ is the deflection on the galvanometer when the helix was moved from the tenth to the eleventh inch from the zero-point; and so we may consider it as the value of Q_ϵ at $10\frac{1}{2}$ inches; so that the values of Q_ϵ refer to the half inches, but Q to the even inches.

In all the calculations the constants in the formulæ were taken to represent Q most nearly, and then the corresponding formulæ for Q_ϵ taken with the same constants.

For ease in calculating by ordinary logarithmic Tables, we may put $\epsilon^{rL} = 10^{·4343rL}$.

IV.

Table I. is from a bar $17\frac{1}{2}$ inches long with a magnetizing helix $1\frac{1}{2}$ inch long at one end, the zero-point being at the other. Table II. is from a bar 9 feet long with a helix $4\frac{1}{2}$ inches long

quite near one end, the zero-point being at 1 inch from the helix toward the long end.   Table III. is from a bar 2 feet long with a helix $4\frac{1}{2}$ inches long near one end, so that its centre was $19\frac{3}{4}$ inches from the end on which the experiments were made, the zero-point being at the end.

In adapting the formula to apply to the case of Table I., we may assume that at the end of the bar $s=\infty$ and $C=0$, which is equivalent to assuming that the number of lines of induction which pass out at the end of the rod are too small to be appreciated.

TABLE I.

Bar ·18 inch diameter.   0 at end of bar.

L.	Q'_ϵ. Observed.	Q'_ϵ. Calculated.	Error of Q'_ϵ.	Q'. Observed.	Q'. Calculated.	Error of Q'.
0	2·7	......	......	0	0	0
3	3·2	......	......	2·7	3·5	+ ·8
5	2·0	2·0	0	5·9	6·6	+ ·7
6	2·5	2·4	− ·1	7·9	8·6	+ ·7
7	3·2	2·8	− ·4	10·4	11·0	+ ·6
8	3·7	3·5	− ·2	13·6	13·8	+ ·2
9	4·3	4·3	0	17·3	17·3	0
10	5·3	5·2	− ·1	21·6	21·6	0
11	6·5	6·5	0	26·9	26·8	− ·1
12	7·7	8·0	+ ·3	33·4	33·3	− ·1
13	9·5	9·9	+ ·4	41·1	41·3	+ ·2
14				50·6	51·2	+ ·6

$$Q' =2{\cdot}60(\epsilon^{{\cdot}213L}-\epsilon^{-{\cdot}213L}).$$
$$Q'_\epsilon=r2{\cdot}60(\epsilon^{{\cdot}213L}+\epsilon^{-{\cdot}213L})={\cdot}554(\epsilon^{{\cdot}213L}+\epsilon^{-{\cdot}213L}).$$

In Table II. observations were not made over the whole length of the rod, and the zero-point was not at the end of the bar. It is evident, however, that by giving a proper value to s we may suppose the bar to end at any point.   As the rod is very long, expressions of the form

$$Q'-C''=C'\epsilon^{-rL}-C'' \text{ and } Q'_\epsilon=rC'\epsilon^{-rL}$$

will apply.

TABLE II.

Bar ·39 inch diameter.   0 at 1 inch from helix.

L.	Q'_ϵ. Observed.	Q'_ϵ. Calculated.	Error of Q'_ϵ.	$Q'-C''$. Observed.	$Q'-C''$. Calculated.	Error of Q'.
0	......	......	......	......	902·5	
1	71·7	70·8	− ·9	825·2	825·9	+ ·7
2	65·2	65·3	+ ·1	753·5	755·1	+1·6
3	59·5	60·2	+ ·7	688·3	689·8	+1·5
4	53·5	55·5	+2·0	628·8	629·5	+ ·7
5	51·2	51·2	0	575·3	574·3	−1·0
6	46·7	47·2	+ ·5	524·1	523·1	−1·0
7	43·2	43·5	+ ·3	477·4	476·0	−1·4
8	40·0	40·1	+ ·1	434·2	432·5	−1·7
9	37·2	37·0	− ·2	394·2	392·5	−1·7
10	34·7	34·1	− ·6	357·0	355·6	−1·4
11	31·7	31·4	− ·3	322·3	321·5	− ·8
12	29·5	28·9	− ·6	290·6	290·1	− ·5
13	25·7	26·6	+ ·9	261·1	261·2	+ ·1
14	25·5	24·6	− ·9	235·4	234·5	− ·9
15	22·0	22·7	+ ·7	209·9	210·0	+ ·1
16	21·5	20·9	− ·6	187·9	187·3	− ·6
17	20·0	19·3	− ·7	166·4	166·4	0
18	19·1	17·8	−1·3	146·4	147·1	+ ·7
19	32·5	31·5	−1·0	127·3	129·4	+2·1
21	27·5	26·7	− ·8	94·8	97·8	+3·0
23	23·0	22·8	− ·2	67·3	71·1	+3·8
25	18·5	19·4	+ ·9	44·3	48·6	+4·3
27	14·5	16·5	+2·0	25·8	29·0	+3·2
29	11·3	14·0	+2·7	11·3	12·6	+1·3
31				0	−1·2	−1·2

$$Q'-C''=983\epsilon^{-·08135L}-80·5=983(10)^{-·0353L}-80·5.$$

$$Q'_\epsilon=r983\epsilon^{-·08135L}\Delta L=80(10)^{-·0353L}\Delta L.$$

In Table II. the observations were near the end of the rod, and were repeated several times. Neglecting the end of the rod, we have $s=\infty$.

In these Tables we see quite a good agreement between theory and observation; but on more careful examination we observe a certain law in the distribution of errors. Thus in Table I. the errors of Q' are all positive between 0 and 8 inches; and this has always been found to be the case at this part of the bar in all my experiments.

The explanation of this is very simple. In obtaining the formulæ, we assumed that the magnetic permeability of the bar μ was a constant quantity; but it has been shown by Dr. Stoletow and myself, independently of each other, that μ increases as the magnetism of the bar increases when the latter is

not great.   Hence between 0 and 8 inches the resistance of the bar, R, is greater than at succeeding points, and hence a less number of lines of induction pass down the bar from 8 towards 0 than would be given by the formula, which has been adapted to the average value of R at from 9 to 14 inches.   In Table II. this same fact shows itself towards the end of the Table, and would probably be more prominent had the Table been carried further.   However, in this Table all things have combined to satisfy the formula with great accuracy.

Table III.

Bar ·39 inch diameter.   0 at end of bar.

L.	Q'_ϵ. Observed.	Q'_ϵ. Calculated.	Error of Q'_ϵ.	Q'. Observed.	Q'. Calculated.	Error of Q'.
0				0·	0	0
1	19·7	15·2	—4·5	19·7	15·2	—4·5
2	16·3	15·3	—1·0	36·0	30·5	—5·5
3	16·0	15·5	— ·5	52·0	46·0	—6·0
4	15·8	15·9	+ ·1	67·8	61·8	—6·0
5	16·5	16 3	— ·2	84·3	78·1	—6·2
6	17·0	16·9	— ·1	101·3	95·0	—6·3
7	17·6	17·6	0	118·9	112·6	—6·3
8	18·4	18·4	0	137·3	130·9	—6·4
9	19·2	19·4	+ ·2	156·5	150·3	—6·2
10	20·3	20·5	+ ·2	176·8	170·7	—6·1
11	21·8	21·7	— ·1	198·6	192·2	—6·4
12	22·8	23·1	+ ·3	221·4	215·3	—6·1
13	24·8	24·7	— ·1	246·2	239·9	—6·3
14	26·8	26·5	— ·3	273·0	266·4	—6·6
15	28·8	28·4	— ·4	301·8	294·6	—7·2
16	31·8	30·5	—1·3	333·6	325·1	—8·5

$$Q'_\epsilon = 7\cdot6(10^{\cdot037\mathrm{L}} + 10^{-\cdot037\mathrm{L}}); \quad Q' = 89(10^{\cdot037\mathrm{L}} - 10^{-\cdot037\mathrm{L}}).$$

In Table III. we come across a fact of an entirely different nature from the above.   Fig. 2 is the plot of this Table, and gives the values of Q'_ϵ at different parts of the rod.

Fig. 2.
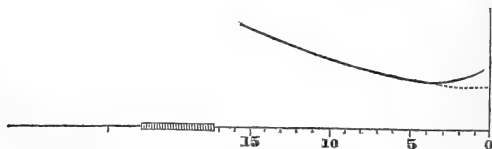
Distribution at end of bar.

The horizontal line in the figure represents values of L,

and the vertical ordinates are values of Q'_e. The full line gives the observed distribution, and the dotted line that according to the formula.

The formula gives the distribution very nearly for all points except those near the end. The formula indicates that Q'_e decreases continually toward the end; but by experiment we see that it increases near this point. On first seeing this, I thought that it was due to some residual magnetism in the bar; but after repeating the experiment several times with proper care, I soon found that this was always the case. I give the following explanation of it :—In the formulæ we have assumed R', the resistance of the medium, to be a constant; now this resistance includes that of the lines of force as they pass from the rod through the medium and thus back to the other end of the rod; and of this whole quantity the part which affects the relative distribution at any part of the rod most is that of the medium immediately surrounding that part; and so the parts near the end have the advantage over those further back, inasmuch as the lines can pass forward as well as outward into the medium. The same thing takes place in the case of the distribution of electricity, where the "density" is inversely proportional to the resistance which the lines of inductive force experience from the medium; and here we find that the "density" is greatest on the projections of the body, showing that the resistance to the lines of induction is less in such situations, and by analogy showing that this must also be the case for lines of magnetic force. But this effect is not very great in cylinders until quite near the end; for Coulomb, in a long electrified cylinder, has found the density at one diameter back from the end only 1·25 times that at the centre; and so there is probably a long distance in the centre where the density is sensibly constant. Hence we may suppose that our second hypothesis, that R' is a constant, will be approximately correct for all parts of a bar except the ends, though of course this will vary to some extent with the distribution of the lines in the medium; at least the change in R' will be gradual except near the end, and so may be partially allowed for by giving a mean value to r.

Hence we see that could the formula be so changed as to include both the variation of R and of R', it would probably agree with the three Tables given.

To study the effect of variation in the permeability more carefully, we can proceed in another manner, and use the formulæ only to get the value of r at different parts of the rods.

No matter how r may vary, equations (2) and (3) will apply to a very small distance l along the rod; and as the origin of coordinates may be at any point on the rod, if Q' and Q'_e are

taken at one point and Q and Q_ϵ at another point whose distance from the first is l, we shall have the four equations

$$Q = C, \qquad Q' = \frac{C}{A+1}(A\epsilon^{rl} + \epsilon^{-rl}),$$

$$Q_\epsilon = \frac{A-1}{A+1}Crl, \qquad Q'_\epsilon = rl\frac{C}{A+1}(A\epsilon^{rl} + \epsilon^{-rl}).$$

Calling $\dfrac{Q'_\epsilon}{Q} = H$ and $\dfrac{Q'}{Q} = G$, we shall find, on eliminating

C and A and developing ϵ^{rl} and ϵ^{-rl},

$$r^2 = \frac{2}{l^2}\left(\frac{GH+1}{G+H} - 1\right), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (9a)$$

or, to a greater degree of approximation,

$$r^2 = \frac{1}{l^2}\left(\sqrt{12\left(2\frac{GH+1}{G+H}+1\right)} - 6\right). \quad \cdot \quad \cdot \quad (9b)$$

Before applying these formulæ to any series of observations, the latter should be freed from most of the irregularities due to accidental causes. For this purpose the following Tables have been plotted and a *regular* curve drawn to represent as nearly as possible the observations; in other cases a column of differences was formed and plotted. In either case the ordinates of the curves were accepted as the true quantities. But, for fear that some might accuse me of tampering with my observations, I have in all cases added these as they were obtained.

The correction is necessary, because small irregularities in the observations will produce immense changes in r^2.

Table IV. contains some of the best observations I have obtained. It is from a bar 57 inches long with a helix $1\frac{1}{2}$ inch long in the centre to magnetize it. Each quantity is the mean of six observations, these being made on both ends of the bar and with the current in opposite directions.

In this Table a source of error was guarded against which I have not seen mentioned elsewhere. When a bar of iron is magnetized at any part and the distribution over the rest quickly measured, on being then allowed to stand some time and the distribution again taken, it will have changed somewhat, the magnetism having, as it were, crept down the bar further. Hence in this Table time was allowed for the bar to reach its permanent state.

Table IV.

Bar ·19 inch diameter. 0 at centre of bar.

L.	Q'_ϵ. Observed.	Q'_ϵ. Corrected.	Q'. Corrected.	$r^2 = \dfrac{R}{R'}$	$\dfrac{1}{r^2} = \dfrac{R'}{R}$
1	24·0	24·0	151·7		
2	17·0	17·0	127·7		
3	13·7	13·7	110·7	·041	24·4
4	11·6	11·65	97·0	·0256	39·1
5	10·2	10·15	85·4	·0192	52·1
6	9·0	9·0	75·2	·0168	59·5
7	8·0	8·0	66·2	·0150	66·7
8	7·1	7·15	58·2	·0142	70·4
9	6·4	6·35	51·1	·0150	66·7
10	5·7	5·65	44·7	·0159	62·9
11	4·9	5·0	39·1	·0160	62·5
12	4·4	4·4	34·1	·0167	59·9
13	3·6	3·9	29·7	·0180	55·6
14	3·3	3·4	25·8	·0184	54·3
15	22·4	22·4	22·4	·0184	54·3
28½					

On looking over column 6, which contains the values of $\dfrac{1}{r^2} = \dfrac{R'}{R} = R'a\mu$ (equation 7), we observe that as Q' decreases, the value of $R'a\mu$ first increases and then decreases. Now it is not probable that R' undergoes any sudden change of this sort; and so it is probably due to change in the permeability of the rod. Hence by this method we arrive at the same results as by a more direct and exact method[*]. But by this means we are able to prove in the most unequivocal manner that *magnetic permeability is a function of the magnetization of the iron and not of the magnetizing force.* Hence it is that I have preferred, in my papers on Magnetic Permeability, to consider it in this way in the formulæ and also in the plots, while Dr. Stoletow (in his paper, Phil. Mag. January 1873) plots the magnetizing-function as a function of the magnetizing force.

When we plot the results in this Table with reference to Q' and $R'a\mu$, the effect of the variation of R' is apparent; and we see, on comparing the curve with those given in my paper above referred to, that R' increases as L increases, at least between L=2 and L=8, which is as we should suppose from the arrangement of the apparatus. For this Table I happen to have data for determining Q in absolute measure; and these show that the maximum value of μ should be about where the Table shows it to be.

[*] Phil. Mag. August 1873.

This method of finding the variation of μ is analogous to that of finding conductivity for heat by raising the temperature of one end of a bar and noting the distribution of heat over the bar; indeed the curves of distribution are nearly the same in the two cases.

If it were thought worth while, it would be very easy to obtain a curve of magnetic distribution for a rod and then enclose the whole rod in a helix and determine its curve of permeability. This would give data for determining R′ in absolute measure at every point of the rod.

To complete the argument that the variation of r^2 is in great measure due to that of μ, I have caused the magnetizing force on a bar to vary. Tables V., VI., and VII. are from a bar 9 feet long and ·25 inch in diameter. At the centre a single layer of fine wire was wound for a distance of 1 foot; and the current for magnetizing the bar was sent through this. The zero-point was at the centre of this helix and at the centre of the bar; so that the observations on the first 6 inches include the part of the bar covered by the helix.

The values of Q'_ϵ are the sum of four observations on each end of the bar and with the current reversed. The three Tables are comparable with each other, the same arbitrary unit being used for all.

TABLE V.

Magnetizing current ·176.

L.	Q'_ϵ. Observed.	Q'_ϵ. Corrected.	Q'. Corrected.	$r^2 = \dfrac{R}{R'}$.	$\dfrac{1}{r^2} = \dfrac{R'}{R'}$.	Q''_ϵ. Calculated.
0	2·7	······	······	······	······	2·40
1	6·9	······	······	······	······	7·32
2	12·7	······	······	······	······	12·54
3	18·2	······	······	······	······	18·31
4	24·4	······	······	······	······	24·87
5	32·4	31·7	220·5	······	······	32·38
6	31·5	32·0	188·5	·0190	52·4	
7	28·2	28·2	160·3	·0212	47·2	
8	24·9	24·7	135·6	·0218	45·9	
9	21·4	21·7	113·9	·0236	42·4	
10	18·6	19·0	94·9	·0252	39·7	
11	16·8	16·4	78·5	·0278	36·0	
12	14·2	14·2	64·3	·0311	32·2	
13	12·0	12·0	52·3	·0367	27·2	
14		10·0	42·3	·0404	24·8	
15 ⎱	17·7	8·2	34·1	:0440	22·7	
16 ⎰		6·6	27·5	·0445	22·5	
17 ⎱	11·6	5·1	22·4	·0570	17·5	
18 ⎰						
End.	22·4	22·4				

$Q''_\epsilon = 3·34(10^{·06(q-x)} - 10^{·06x}).$

TABLE VI.
Magnetizing current ·31.

L.	Q'_ϵ Observed.	Q'_ϵ Corrected.	Q' Corrected.	r^2.	$\dfrac{1}{r^2}$.	Q''_ϵ Calculated.
0	16·3	......	......	......	......	17·3
2	22·0	......	......	......	......	22·3
3	32·4	......	......	......	......	32·28
4	43·8	......	......	......	......	43·34
5	55·9	......	391·9	......	......	55·90
6	55·2	55·1	336·8	·0204	49·0	
7	46·8	48·1	288·7	·0201	49·7	
8		42·3	246·4	·0202	49·5	
9	81·3	37·4	209·0	·0220	45·5	
10		33·0	176·0	·0243	41·2	
11	61·8	29·0	147·0	·0262	38·2	
12		25·3	121·7	·0300	33·3	
13	46·4	21·9	99·8	·0352	28·4	
14		18·7	81·1	·0405	24·7	
15	35·4	15·6	65·5	·0479	20·9	
16		12·7	52·8			
17	22·0	9·8				
18						
End.	43·0					

$$Q''_\epsilon = 7\cdot74(10^{\cdot08(b-x)} - 10^{\cdot08x}).$$

TABLE VII.
Magnetizing current 1·12.

L.	Q'_ϵ Observed.	Q'_ϵ Corrected.	Q' Corrected.	r^2.	$\dfrac{1}{r^2}$.	Q''_ϵ Calculated.
0	3·5	......	762·4	......	......	2·58
1	9·4	......	758·9	......	......	8·29
2	15·4	......	749·5	......	......	15·78
3	27·5	......	734·1	......	......	26·70
4	44·3	......	706·6	......	......	43·36
5	66·6	......	662·3	......	......	69·37
6	71·2	71·2	595·7	·0239	41·8	
7	59·5	59·7	524·5	·0200	50·0	
8	51·0	51·2	464·8	·0162	61·7	
9	45·2	45·2	413·6	·0141	70·9	
10	40·3	40·3	368·4	·0120	83·3	
11	36·3	36·8	328·1	·0107	93·5	
12	33·3	33·5	291·3	·0110	90·9	
13	30·6	30·5	257·8	·0116	86·2	
14	28·1	28·0	227·3	·0118	84·7	
15	25·6	25·4	199·3	·0140	71·4	
16	23·4	22·7	173·9	·0147	68·0	
17	20·0	20·3	151·2	·0161	62·1	
18		18·1	130·2	·0180	55·6	
19	34·0	16·0	112·8			
20			96·8			
End.	96·8					

$$Q''_\epsilon = \cdot35(10^{\cdot02(b-x)} - 10^{\cdot02x}).$$

Here we see an excellent confirmation of the results deduced from Table IV. In Table V., where the magnetizing force is very small, and where, consequently, no part of the iron has yet reached its minimum resistance, the value of $\frac{1}{r^2} = \frac{R'}{R} = R'a\mu$ decreases continually as the value of Q' decreases, as it should do. In Table VI., with a higher magnetizing power, which was sufficient to bring a portion of the bar to about the minimum resistance, we see that $\frac{1}{r^2}$ remains nearly stationary for a short distance from the helix and then decreases in value. In Table VII., where the bar is highly magnetized and the portion near the zero-points approaches the maximum of magnetization, $\frac{1}{r^2}$ increases in value as we pass down the bar ; and having reached its maximum at $L = 11\frac{3}{4}$ nearly, it decreases. These Tables, then, show in the most striking manner the effect of the variation of the magnetic permeability of iron upon the distribution of magnetism.

It is evident that these Tables also give the data for obtaining the relative values of R' at different parts of the bar; but the results thus obtained are conflicting, and will need further experiment to obtain accurate results. Where such a small magnetizing force is used as in Table V. it is almost impossible to attain accuracy ; and allowance should be made for this in deducing results from it. The greatest liability to error is of course where the magnetization is small; for any small residual magnetism which the bar may contain will be more apparent here—although great care was taken to remove all residual magnetism before use. Besides this there are many other disturbances from which the higher magnetizing powers are free.

If we accept Green's formula as correct, *these observations give us data for determining the magnetizing-function of iron in a unique manner*, for nearly all other methods depend on absolute measurements of some kind. Thus the least value of r^2 in Table IV. for a rod ·19 inch diameter is ·0142, which gives $p = ·01132$, which in Green's formula (equation 8) gives $\mu = 3388$ for the greatest permeability of this iron ; and this is as nearly right as we can judge for this kind of iron. It is to be noted that Green's formula has been found for the portion of the bar covered by the helix ; but, as seen from my formulæ, it will approximately apply to all portions, though it would be better to find a new formula for each case.

We shall, toward the last, resume this subject again ; and so will leave it for the present.

The results which I have now given, and indeed all the results of this paper, have been deduced not only from the observations which I publish, but from very many others ; so that my Tables may be *considered to represent the average* of a very extended series of researches, though they are not really so.

<div style="text-align:center">[To be continued.]</div>

XXX. *On a controverted Point in* Laplace's *Theory of the Tides.* *By* Sir George B. Airy, *Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN a paper published in the last Number of the Philosophical Magazine, Sir William Thomson has, with great personal courtesy towards myself, objected to my imputation (in the article "Tides and Waves" of the *Encyclopædia Metropolitana*) to Laplace, of a serious error in a portion of his investigation of the theory of the Tides. I had at first proposed to myself to enter into a discussion of Sir William Thomson's methods ; but I soon found my thoughts taking the same course as in the original article ; and I finally judged it best to do little more than to refer to that article, with perhaps a slight expansion of the verbal remarks.

The part of the *Mécanique Céleste* in which the investigation in question occurs is Livre IV., Chapitre Premier, Article 10 ; and the part of my own Essay, in which I have endeavoured to put Laplace's investigation into a clearer form, is Section III. Art. 110. And the following is the course of the process. Supposing the depth of the sea to be uniform, and making (for the present) no assumption regarding its boundaries, there is to be satisfied a certain differential equation of which every term can be expressed as a multiple of an even power of $\sin \theta$. The quantity to be determined is called ga. To obtain its value by the method of indeterminate coefficients, the form is assumed,

$$ga = \mathrm{K}_2 \sin^2 \theta + \mathrm{K}_4 \sin^4 \theta + \&\text{c.} + \mathrm{K}_{2k} \sin^{2k}\theta + \mathrm{K}_{2k+2} \sin^{2k+2}\theta + \&\text{c.},$$

or, if $ga = \mathrm{G} \sin^2 \theta + a'''$,

$$a''' = (\mathrm{K}_2 - \mathrm{G}) \sin^2 \theta + \mathrm{K}_4 \sin^4 \theta + \&\text{c.} + \mathrm{K}_{2k} \sin^{2k}\theta \\ + \mathrm{K}_{2k+2} \sin^{2k+2}\theta + \&\text{c.}$$

Substituting this in the differential equation above mentioned, and comparing the coefficients of the different powers of $\sin \theta$, the following subordinate equations are obtained :—

$$8(K_2 - G) = 0;$$
$$12(K_4 - K_4) = 0;$$
$$(-16K_6 + 10K_4)\frac{l}{bm} - 4K_2 = 0;$$
$$(-40K_8 + 28K_6)\frac{l}{bm} - 4K_4 = 0;$$

and generally, after the first two,

$$\left\{ -(4K^2 + 12k)K_{2k+4} + (4k^2 + 6k)K_{2k+2} \right\}\frac{l}{bm} - 4K_{2k} = 0.$$

Now an equation like this, which gives the third term in terms of the first and second, the fourth term in terms of the second and third, and so on, must have the first and second terms as foundations or origins wherefrom to start for the formation of the entire series. There is no more ground for inferring the second term from the general subordinate equation than for inferring the first term from it. The first and second terms must stand on their own foundation. The value of K_2 is perfectly clear, namely $K_2 = G$ (not deduced from the general subordinate equation). But how with regard to K_4? The subordinate equation $12(K_4 - K_4) = 0$ gives no information whatever; and I infer from this that we may give to K_4 any value that we think fit (as regards the equation, unlimited by boundaries &c., with which we started), or any value that may be required by physical circumstances not yet introduced (as the boundary of the sea). But does any arbitrary value of K_4 really satisfy the original differential equation? It does. Then I say that K_4 is truly an undetermined quantity, which may perhaps be determined hereafter by reference to physical conditions not yet stated, but which cannot under any circumstances be inferred from the general subordinate equation. And, so far as I know, the whole course of applied algebra does not offer a more certain conclusion. I scarcely need to add that I look upon Laplace's process as a mere sport with symbols, and upon Laplace's conclusion as a grievous error.

In Article 112 of my Essay, I have given the series which ought to be added to Laplace's series in order to make the solution complete; and I now repeat it as accurate (chance-errors of numbers excepted). The additional series is the following:—

$$K_4 \times \left\{ \sin^4\theta + \frac{5}{8}\sin^6\theta + \left(\frac{7}{16} - \frac{bm}{10l}\right)\sin^8\theta \right.$$
$$\left. + \left(\frac{189}{576} - \frac{79}{720}\frac{bm}{l}\right)\sin^{10}\theta + \&c. \right\},$$

where K_4 is absolutely indeterminate as regards the general solution of the original equation (the only solution of which Laplace is treating here), and where of course we may put K_4 or $\dfrac{K^4}{g}$ as may be convenient. And in Article 113 I have pointed out that the value of K_4, undetermined in the general solution of the original equation, may be so determined as to apply to a sea bounded on its north or south side. And this is the legitimate use of such an undetermined constant.

<div align="right">G. B. AIRY.</div>

Royal Observatory, Greenwich,
September 11, 1875.

XXXI. *Note on the " Oscillations of the First Species" in* Laplace's *Theory of the Tides.* By Sir WILLIAM THOMSON, F.R.S.*

LAPLACE'S "Oscillations of the First Species" are simple harmonic oscillations, in which the surface of the water is always a figure of revolution round the axis of rotation. The "tide-generating influence" in this case is such that the equilibrium tide-height would be $\Theta \cos \sigma t$ at time t, σ denoting a constant (called the "speed" in the British-Association Tidal Committee's Report for 1871), and Θ a function of the latitude. Θ being supposed known, the problem consists in finding a' a function of the latitude such that $(\Theta + a') \cos \sigma t$ is the actual tide-height at time t, and, for the case of the sea equally deep everywhere, is to be solved by finding the proper solution of the differential equation

$$\frac{d}{d\mu}\left(\frac{1-\mu^2}{\mu^2-f^2}\frac{da'}{d\mu}\right) - 4ea' = 4e\Theta ; \quad . \quad . \quad . \quad . \quad (1)$$

where μ denotes the sine of the latitude, and e and f are constants defined by the equations

$$f = \frac{\sigma}{2n}, \quad e = \frac{n^2 r^2}{g\gamma} = \frac{mr}{\gamma} ;$$

r being the earth's radius,

g the force of gravity at its surface,

m the ratio of gravity to equatorial centrifugal force, being equal to $\frac{1}{289}$,

n the angular velocity of the earth's rotation,

and γ the depth of the sea, supposed small in comparison with r—not greater, say, than $\frac{1}{50}r$.

The quickest of the "Oscillations of the First Species" is the

lunar fortnightly (declinational); and for it σ is about $\frac{1}{14}$ of $\dot{n}$, which makes $f = \frac{1}{28}$. Even for this, and more decidedly for the lunar monthly (elliptic) and solar semiannual (declinational) and annual (elliptic), a good approximation to the result might be obtained by taking $\sigma = 0$. Laplace does not enter on the integration of the equation, but contents himself by pointing out that an infinitesimal degree of friction will, when $\sigma = 0$, cause the actual tide-height to be the same as the equilibrium tide-height, and that even for the lunar fortnightly the actual height must be sensibly the same as the equilibrium height if there is enough of friction to reduce in a fortnight a free oscillation to a small fraction of its original amount. The result of *any* tide-generating influence of sufficiently long period would obviously be more and more nearly in exact agreement with the equilibrium theory the longer the period, were it not for the earth's rotation. But, because of the earth's rotation, a long-period tide does not approximate to agreement with the equilibrium tide if the water be perfectly frictionless; and the solution of the beautiful "vortex problem" thus presented is what is aimed at by Airy[*] and Ferrel[†] in their integration of the preceding equation for the case $\sigma = 0$, in which it is reduced to the comparatively simple form [‡]

$$\frac{d}{d\mu}\left(\frac{1-\mu^2}{\mu^2}\frac{da'}{d\mu}\right) - 4eu = 4e\Theta \quad . \quad . \quad . \quad (2)$$

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[*] " Tides and Waves " (*Encyclopædia Metropolitana*), art. (97).

[†] "Tidal Researches " (Appendix to United-States Coast-Survey Report, 1874), § 151.

[‡] [Note added, Bristol, September 2, 1875.]—Without this simplification, the equation (1) is susceptible of nearly as simple a solution as with it. Assume

$$\frac{1}{\mu^2-f^2}\frac{da'}{d\mu} = \Sigma K_i \mu^i.$$

This gives

$$a = \Sigma \frac{\mu^i}{i}(K_{i-3} - f^2 K_{i-1})$$

and

$$\frac{d}{d\mu}\left(\frac{1-\mu^2}{\mu^2-f^2}\frac{da'}{d\mu}\right) = \Sigma\mu^i(i+1)(K_{i+1} - K_{i-1});$$

so that, to determine the coefficients K_i, we have the equation of condition

$$(i+1)K_{i+1} - \left(i+1-\frac{4ef^2}{i}\right)K_{i-1} - \frac{4e}{i}K_{i-3} = \Theta_i,$$

if

$$\Theta = \Sigma\Theta_i\mu^i.$$

This is a particular case of an almost equally simple solution of Laplace's general equation of Tides, which has been communicated to the British Association at its meeting now concluded, and will be published also in the November Number of the Philosophical Magazine.]

[which substantially agrees with Airy's equation of art. (97) (with $q=0$, to make the depth constant as we now suppose it), and with Ferrel's § 151, equation (288); but is simpler in form, partly through the use of Laplace's notation μ for $\cos\theta$]. For each of the "long-period tides" in the actual case of the earth under the influence of the sun and moon, the function Θ is given by the formula

$$\Theta = H(1-3\mu^2), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

where H denotes the equilibrium value of the tide-height at the equator. Airy, with this value of Θ, finds an integral of the differential equation by assuming

$$a' = B_2\mu^2 + C_4\mu^4 + \ldots B + \&c.,$$

and determining the coefficients so as to satisfy it. But this assumption errs in making the tide-height at the equator equal to the equilibrium height. The correct assumption for the particular problem proposed (or for any case in which Θ involves only even powers of μ) is

$$a^i = B_0 + B_2\mu^2 + B_4\mu^4 + \ldots;$$

but the more general assumption,

$$a' = B_0 + B_1\mu + B_2\mu^2 + \ldots + B_i\mu^i + \ldots, \quad \cdot \quad \cdot \quad (4)$$

is as easily dealt with (and includes oscillations in which the equator is a line of nodes). With it we have

$$\frac{d}{d\mu}\left(\frac{1-\mu^2}{\mu^2}\frac{da'}{d\mu}\right) - 4ea' = \sum_{i=-2}^{i=\infty}\mu^i\{(i+4)(i+1)B_{i+4}$$
$$-(i+2)(i+1)B_{i+2} - 4eB_i\},$$

which is to be equated to $4e\Theta$. Thus, for the case of

$$\Theta = H(1-3\mu^2),$$

we find, by putting $i=-2$, $i=0$, $i=2$, &c. :—

$$\left.\begin{array}{l} 2 \cdot (-1) \cdot B_2 = 0, \\ 4 \cdot 1 \cdot B_4 - 2 \cdot 1 \cdot B_2 - 4eB_0 = 4eH, \\ 6 \cdot 3 \cdot B_6 - 4 \cdot 3 \cdot B_4 - 4eB_2 = -12eH, \end{array}\right\} \quad \cdot \quad \cdot \quad (5)$$

and

$$(i+4)(i+1)B_{i+4} - (i+2)(i+1)B_{i+2} - 4eB_i = 0, \quad \cdot \quad (6)$$

for all even positive values of i except 0 and 2.

The first of equations (5) gives $B_2 = 0$; and with this the second and third give

$$\left.\begin{array}{l} B_4 = e(B_0 + H), \\ B_6 = \tfrac{2}{3}eB_0; \end{array}\right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (7)$$

and if in (6) we put successively $i=4$, $i=6$, $i=8,\ldots$ and use

in this order the equations so found, we can calculate successively by means of them, B_8, B_{10}, B_{12}, . . . each in terms of B_0; and we thus have a solution of (2), with one arbitrary constant, B_0, which may be written thus,

$$a' = H . f(\mu, e) + B_0 . F(\mu, e), \quad . \quad . \quad . \quad (8)$$

where $f(\mu, e)$ denotes the function of μ and e expressed by the series (4), with the coefficients calculated for the case $B_0 = 0$ and $H = 1$; and $F(\mu, e)$ the function similarly found by taking $H = 0$ and $B_0 = 1$.

The constant B_0, as Airy has pointed out [Tides and Waves, art. (113)] with reference to a corresponding question in the solution for semidiurnal tides, may be assigned so as to make the north and south component motion of the water zero in a given latitude. In the present case (that is, the case of symmetry round the axis of rotation) we have [Airy, art. (95), or Laplace, Liv. iv. chap. i. art. (3)]

northward displacement of water $= \dfrac{\cos \sigma t}{4m \sqrt{(1 - \mu^2)}} \dfrac{da'}{d\mu};$. (9)

and therefore to make the north and south motion zero we must have

$$\frac{da'}{d\mu} = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

whence, by (8),

$$B_0 = H . \frac{- \dfrac{df(\mu, e)}{d\mu}}{\dfrac{dF(\mu, e)}{d\mu}}. \quad . \quad . \quad . \quad . \quad (11)$$

If, then, we find B_0 by this equation for any given value of μ, we have a solution of the determinate problem of finding the motion of the water under the given tide-generating influence when, instead of covering the whole earth, the sea covers only an equatorial belt between two equal circular polar islands.

The solution thus obtained is in a series essentially convergent, except in the extreme case of the polar islands vanishing. For, taking the equations (6) in the order indicated above, and so calculating B_2 successively from smaller to greater values of i by the formula

$$B_{i+4} = \frac{i+2}{i+4} B_{i+2} - \frac{4e B_i}{(i+4)(i+1)}, \quad . \quad . \quad (12)$$

we inevitably find for greater and greater values of i,

$\dfrac{B_{i+4}}{B_{i+2}} = \dfrac{i+2}{i+4}$, more and more nearly the greater is i, . (13)

and this whatever value, zero or other, we give to B_0 (unless we give it *precisely* the value found by Laplace's method below, and then perform each step of the calculation with infinite accuracy). Hence, whatever be the value of e, the series expressing the solution converges for every value of $\mu < 1$. Thus the solution is thoroughly satisfactory for the supposed case of two equal polar islands of any finite magnitude. But the ultimate convergence is shown by (13) to be the same as that of the series

$$\frac{\mu^2}{1} + \frac{\mu^4}{2} + \dots \frac{\mu^{2i}}{i} + \dots,$$

which is equal to

$$\log \frac{1}{1-\mu^2}.$$

Hence, when $\mu = 1$, the series for a' becomes infinitely great; and *à fortiori* it gives an infinitely great value for $\frac{da'}{d\mu}$, unless it has been calculated for *precisely* the particular value of B_0 sought. Hence equation (11) fails to determine this value. Thus the solution fails for the very case for which it was sought, the case proposed originally by Laplace, and taken by Airy and Ferrel as the subject of their investigation—that is, the case of the whole earth covered with water. Here Laplace's brilliant process, referred to in an article in the preceding Number of the Philosophical Magazine, comes to our aid marvellously.

Let

$$\frac{B_{i+2}}{B_i} = \frac{-1}{N_i}. \quad . \quad . \quad . \quad . \quad . \quad (14)$$

We have, by (6),

$$N_i = \frac{1}{4e} \left\{ (i+2)(i+1) + \frac{(i+4)(i+1)}{N_{i+2}} \right\}. \quad . \quad (15)$$

From this equation applied to any moderately great even value of i (greater or less great according to the degree of approximation required), taking $N_{i+2} = \infty$, calculate N_i, and then, by successive applications for smaller even values of i in order, calculate $N_{i-2}, N_{i-4}, \dots N_6, N_4$ successively. Equations (7), with

$$B_4 = -N_4 B_6, \quad . \quad . \quad . \quad . \quad . \quad (16)$$

then give

$$B_0 = -\frac{3H}{3 + 2N_4},$$

$$B_4 = e\frac{2N_4 H}{3 + 2N_4}, \qquad \left. \begin{array}{c} \\ \\ \\ \\ \end{array} \right\} \quad . \quad . \quad . \quad (17)$$

and

$$B_6 = -e\frac{2H}{3 + 2N_4}.$$

Thus, finally, the solution is

$$a' = \frac{2e\mathrm{H}}{3+2\mathrm{N}_4}\left\{ -\frac{3}{2e} + \mathrm{N}_4\mu^4 - \mu^6 + \frac{\mu^8}{\mathrm{N}_6} - \frac{\mu^{10}}{\mathrm{N}_6 \cdot \mathrm{N}_8} + \frac{\mu^{12}}{\mathrm{N}_6 \cdot \mathrm{N}_8 \cdot \mathrm{N}_{10}} - \&\mathrm{c.} \right\}, \quad (18)$$

where N_4, N_6, N_8, ... are functions of e determined by (15).

Yacht 'Lalla Rookh,'
Largs, August 14, 1875.

XXXII. *The Augmentation of the Chemical Activity of Aluminium by Contact with a more negative Metal.* By J. H. GLADSTONE, *Ph.D., F.R.S., Fullerian Professor of Chemistry in the Royal Institution, and* ALFRED TRIBE, *Esq., Lecturer on Chemistry in Dulwich College*[*].

HAVING had occasion to employ aluminium for effecting certain decompositions, we were induced to determine whether covering its surface with more-negative metals would so augment its chemical activity as to enable it to decompose water below or at 100° C.

According to Deville, the metal *alone* does not decompose water except at a white heat.

The following experiments were made.

(*a*) 0·43 grm. of metallic copper was deposited upon 6 grms. of aluminium-foil (126 centims. long, 5 centims. wide) in a two-ounce flask by immersion in very dilute copper-sulphate solution very slightly acidulated with hydrochloric acid. The couple so obtained was completely freed from sulphate and chloride by washing with pure water.

(*b*) 0·2214 grm. of platinum was deposited upon a similar quantity of aluminium-foil by immersion in dilute platinic chloride and was washed as in the former instance.

The flasks containing these preparations were filled with water and allowed to stand at the temperature of the air for twenty-two hours, when they were heated to 100° C. Hydrogen gas was evolved.

The results obtained were as follows :—

Temperature.	Time.	Al-Cu experiment. Hydrogen.	Al-Pt experiment. Hydrogen.
		cub. centim.	cub. centim.
$1\overset{\circ}{2}$ C.	22 hours	2·5	4
100	next 6 ,,	375	484
,,	,, ,,	92	114
,,	,, ,,	55	78
,,	,, ,,	33	45

The action proceeded, at a diminishing rate, for several days.

The fact brought out by the above experiments was further established by other decompositions, and is of course corroborative of our work with zinc conjoined to more negative metals in a spongy condition.

XXXIII. *Notes on the Action of the Copper-Zinc Couple.* By Professor GLADSTONE *and* ALFRED TRIBE[*].

Relative Activity of pure Zinc and Zinc covered with spongy Copper.

A SOLUTION of sulphuric acid containing $3\frac{1}{2}$ parts of acid to 1000 of water is just acted upon by pure zinc.

In an experiment with 2·5 grms. pure zinc (granulated) immersed in this strength of acid, 7 volumes of hydrogen were given off in one hour.

In another experiment, in which the same piece of zinc upon which ·003 grm. of copper had been deposited was used, 80 volumes of hydrogen were given off in the same time.

Thus it appears that the activity of zinc in very dilute sulphuric acid is increased elevenfold by 0·12 per cent. of the negative metal.

Arseniuretted Hydrogen.

If zinc containing arsenic act upon dilute sulphuric acid, the hydrogen, as is well known, contains AsH_3, the formation of which is explicable (from analogy with what is known of the action of hydrogen upon oxynitrogen compounds) on the supposition that the arsenic becomes dissolved, and that by the subsequent action of hydrogen upon the arsenical compound in solution the arseniuretted hydrogen is produced.

Some four years ago we pointed out that the copper-zinc couple

in presence of water effects the decomposition of that fluid—zinc-hydrate and hydrogen being produced.

According to Bonsdorff, arsenic does not dissolve in water free from dissolved oxygen—a fact which we have verified, and also that dilute sulphuric acid does dissolve it.

If the foregoing view of the formation of AsH_3 be correct, the hydrogen obtained by the action of the couple upon water should be free from arsenical gas, even though the couple be made with arsenical zinc.

We give the results of four experiments. A quantity of arsenical zinc-foil was "coupled" with copper, washed, and heated with water; and two litres of gas evolved were passed through a tube heated to redness. Not a trace of arsenic was noticeable.

A portion of the same foil not "coupled" was treated with dilute sulphuric acid, and two litres of hydrogen evolved by the action were passed through a tube heated as before. 0·0019 grm. of arsenic was deposited in the cool part of the tube.

The arsenical zinc with a covering of spongy copper, when treated with dilute sulphuric acid, gave arseniuretted hydrogen, which appears to point to the conclusion that it is not the copper, but the inability of the arsenic to get into solution, that accounts for the absence of hydrogen in the gas from water and the couple—a conclusion that was confirmed by adding a dilute solution of arsenic (1 part As_2O_3 in 12,000 of water) to some couple, when immediately the arsenical mirror was produced in the heated tube.

XXXIV. *The Wind Theory of Oceanic Circulation.—Objections examined.* By JAMES CROLL, *of H. M. Geological Survey of Scotland* *.

IN my paper in the Philosophical Magazine for September, it was shown by the temperature-soundings of the 'Challenger' Expedition that the surface of the North Atlantic must be above the level of the equator, and that consequently, in so far as that ocean is concerned, it is mechanically impossible that there can be any such surface-flow as is assumed in the gravitation theory. As this striking result has been questioned by some physicists, I herewith subjoin Muncke's Table of the expansion of sea-water, from which any one can readily satisfy himself as to the accuracy of my figures.

* Communicated by the Author.

TABLE of the Expansion of Sea-water according to the Experiments of Professor Muncke, of Heidelberg. From Twelfth Number of ' Meteorological Papers, published by the Board of Trade.'

Temp.	Expansion.	Temp.	Expansion.	Temp.	Expansion.
32	1·00000	49	1·00098	66	1·00279
33	1·00004	50	1·00107	67	1·00292
34	1·00007	51	1·00116	68	1·00305
35	1·00011	52	1·00125	69	1·00319
36	1·00015	53	1·00134	70	1·00332
37	1·00020	54	1·00143	71	1·00347
38	1·00025	55	1·00153	72	1·00361
39	1·00030	56	1·00163	73	1·00375
40	1·00036	57	1·00173	74	1·00390
41	1·00041	58	1·00184	75	1·00406
42	1·00047	59	1·00195	76	1·00421
43	1·00054	60	1·00207	77	1·00436
44	1·00061	61	1·00218	78	1·00453
45	1·00068	62	1·00238	79	1·00468
46	1·00076	63	1·00242	80	1·00484
47	1·00083	64	1·00254	81	1·00500
48	1·00091	65	1·00266	82	1·00517

The following is the extent by which the three columns A, B, and C (pp. 244, 245) are each lengthened by heat above what they would be were the water of the uniform temperature of 32° Fahr., the expansion of A and B below the level of the bottom of C not being, of course, taken into account.

> Column A, lat. 38° N.... 8·05 feet.
> Column B, lat. 23° N.... 7·22 feet.
> Column C, equator ..... 4·52 feet.

Thus column C (the equatorial) is shorter than column B by 2·70 feet, and than column A by 3·53 feet.

The fundamental arguments of the advocates of the gravitation theory.—1. The gravitation theorists base their argument on two principal assumptions which cannot be conceded. First, they maintain that the existence of polar water in the depths of the ocean is consistent with their theory only; and, secondly, they assume as a necessary condition of the wind theory that the understratum of the ocean should consist of warm water. It is a well recognized fact that the ocean beyond the reach of sun-heat is occupied with water of a polar temperature; and they therefore point triumphantly to this fact as at once a proof of their position and a conclusive argument against the wind theory. But, on the other side, it will not be difficult to show that the existence of cold water throughout the ocean depths is

as much a necessary result of the wind theory as of the gravitation theory, and that there is no relation whatever between the wind theory and warm water in the depths of the sea.

It is supposed that the return *under-currents* from the polar regions are by far too insignificant to be able to maintain at a polar temperature the great depths of the ocean.

Let us examine this objection. It is freely admitted, nay even strenuously maintained by the advocates of the gravitation theory themselves, that the heating-power of the sun does not extend to any great depth below the surface of the ocean; consequently there is nothing whatever to heat this mass of water underneath except the heat coming through the earth's crust; but the amount of heat derived from this source is so trifling that an under-current from the Arctic regions of no great magnitude would be sufficient to keep the mass at an ice-cold temperature.

On a former occasion * I showed that, taking the rate at which internal heat passes through the earth's surface to be that assigned by Sir William Thomson, the total amount received per annum by the North Atlantic, between the equator and tropic of Cancer, including the Caribbean Sea, is equal to only $\frac{1}{894}$ of that conveyed by the Gulf-stream, on the supposition that each pound of water carries 19,300 foot-pounds of heat,—and that consequently an under-current from the polar regions of not more than $\frac{1}{35}$ the volume of the Gulf-stream would suffice to keep the entire mass of water of that area within 1° of what it would be were no heat derived from the crust of the earth, and an under-current of less than $\frac{1}{17}$ that of the Gulf-stream coming from the polar regions would keep the entire North Atlantic from the equator to the arctic circle filled with ice-cold water. A polar under-current half the size of the Gulf-stream would be sufficient to keep the entire water of the globe (below the stratum heated by the sun's rays) at an ice-cold temperature. Internal heat would not be sufficient under such circumstances to maintain the mass 1° Fahr. above the temperature it possessed when it left the polar regions.

In short, whatever theory we adopt regarding oceanic circulation, it follows equally as a necessary consequence that the entire mass of the ocean below the stratum heated by the sun's rays must consist of cold water. For if cold water be continually coming from the polar regions, either in the form of under-currents, or in the form of a general underflow as Dr. Carpenter supposes, the entire under portion of the ocean must ultimately become occupied by cold water; for there is no

* Philosophical Magazine, June 1874; 'Nature,' vol. x. p. 52.

source from which this influx of water can derive heat, save from the earth's crust, which amount is so trifling as to produce no sensible effect.

It is therefore evident that the great mass of cold water occupying the depths of the ocean cannot be urged as an objection to the wind theory.

2. But it is asserted that the impulse of the wind on the surface of the ocean cannot produce and maintain deep under-currents. This is an objection which has been urged by some eminent physicists; but it is based upon a misapprehension of the manner in which, according to the wind theory, under-currents are produced.

It is true, as the objectors maintain, that a wind simply impelling the water forward will not necessarily produce an under-current, since compensation will more readily take place by return surface-currents, as in this case the path of least resistance will generally be at the surface. But when the general surface of one half of an ocean basin is being constantly impelled forward by prevailing winds in a contrary direction to that in which it is being impelled in the other half, compensation cannot possibly take place by means of return surface-currents. For a full discussion of this point I must refer the reader to my work, ' Climate and Time,' Chap. XIII.

It is, however, needless to advance arguments *à priori* against the possibility of such under-currents; for we have actually several well-known examples of such currents, the particulars of which will also be found in the work to which I refer.

3. But supposing it could be shown that the winds cannot directly produce under-currents, it can nevertheless be demonstrated that they can do so indirectly. A vertical circulation filling the deep recesses of the ocean under the equator with polar-cold water, follows as readily and truly from the wind theory as it does from the gravitation theory. It has been shown that the general tendency of the system of the winds is to impel the surface-water of the equatorial regions into the temperate and polar regions as rapidly as it is heated. But such a transference of surface-water must tend to destroy static equilibrium by making the equatorial too light and the temperate and polar columns too heavy, as truly as though the transference had taken place by means of difference of temperature. The effect must be to produce a constant ascent of the equatorial column and an *inflow* of cold water below equal to the *outflow* above. In short, the wind must produce a system of circulation precisely the same as that supposed to take place by difference of temperature.

By both theories the cause of the vertical motion is the transference of water from the top of the one column to the top of the other. This vertical motion is therefore as much a necessary consequence of the wind theory as it is of the gravitation theory.

XXXV. *On Stationary Liquid Waves.*
By Frederick Guthrie[*].

THE following numerical results of experiments may be of use in the further study of wave-motion. They refer to stationary waves mainly, and include those of circular and those of rectangular troughs. The guiding idea was :—to deduce the velocity of wave-progression under different conditions from the frequency of recurrence of a given phase in the same place, in a manner similar to that employed in the measurement of the velocity of a sound-wave through a solid by the pitch of the note when a certain length of the solid vibrates longitudinally ; and, by making use of reflection, to bring into a compact form for study the effects of wave-progression.

Circular Troughs.

§ 1. The first condition of motion examined in circular troughs is that which I shall call binodal. This motion can be easily set up in a cylindrical trough of water if it be not much less than 9 inches in diameter, nor the water much less than 6 inches deep. The bottom of an empty beaker glass serves well to generate the waves, on account of its buoyancy. This is placed on the centre of the water, and moved down and up at a rate dependent upon the diameter of the vessel. The motion of the water soon guides the hand, and, being judiciously humoured, establishes itself and continues for many minutes with great regularity. The number of times in one or more minutes that the crest appears in the middle may be easily counted if the reflection of a window-sash is seen obliquely on the liquid surface.

§ 2. *Influence of amplitude.*—Before examining the influence of diameter (which with stationary waves amounts to an examination of the influence of wave-length), a brief examination of the influence of Depth, Temperature, Density, and Amplitude had to be undertaken, to determine at all events the margin of variation admissible without vitiation of the result. The amplitude was measured by standing a brass wire graduated in millimetres, and fastened to a leaden foot, in various parts of the trough after the wave-system was established.

§ 3. A cylindrical vessel, B, of tin plate had the mean diameter 456 millims. (It was slightly elliptical, 455, 457 millims.)

The depth of the water in it was 290 millims., and its temperature 16° C. Being set in binodal motion, the gauge was put in and the amplitude allowed to subside to 30 millims. at the centre.

In the 1st minute 122 recurrences were counted.

,,   2nd   ,,   122·5   ,,   ,,

,,   3rd   ,,   122   ,,   ,,

There was an interval of 10″ between the 1st and 2nd minute, and an interval of 15″ between the 2nd and 3rd minute. The amplitude of motion at the centre had now sunk to less than 1 millim. This shows that in this system amplitude or wave-height is without sensible influence. This is confirmed by the results of the following experiment, in which the initial amplitude at the centre was again 30 millims., and in which the number of pulsations made in 4′ was counted in four experiments.

(1) Amplitude at first at centre 30 millims.; in 4′ there were 490

(2)   ,,   ,,   ,,   ,,   489·5

(3)   ,,   ,,   ,,   ,,   489·5

(4)   ,,   ,,   ,,   ,,   489

or B_n mean in $1' = 122 \cdot 4$.

§ 4. *Effect of Temperature.*—The water in vessel B was now heated to 41° C., or through 25° C. It had the same depth as before. The following three observations were made:—

(1) In 3′ there were 367 pulsations.

(2)   ,,   ,,   366   ,,

(3) In 3′ 25″   ,,   420   ,,

which gives for 1′ the mean number 122·4, precisely the same result as for T=16°. Temperature, therefore, has no appreciable influence, and its variation may be neglected.

§ 5. *Effect of Depth.*—For the examination of this influence the vessel B was again employed. The maximum amplitude at the centre was in all cases, at the beginning, about 30 millims.

Depth.	Time.	Number of pulsations counted.	$n.$
millim.			
290	4′	489·5	122·4
270	4	490	122·5
250	3	368	122·6
230	4	490	122·5
210	3	368	122·6
190	3	367	122·3
170	3	367	122·3
150	3	366	122·0
130	3	364·5	121·5
110	3	360	120·0
90	2	234	117·0
70	2	225	112·5
50	1	102	102·0

At a depth of 30 millims. no fundamental undulations could be preserved. In order to see whether the effect of shallowness took most effect on waves of larger or smaller amplitude, the depth 70 millims. was restored and an initial central amplitude of 30 millims. was given. In the first minute 113 were counted; after a lapse of 10″ the same number 113 were counted in the second minute. A second experiment, in which the interval between the two minutes was 15″, gave for both minutes exactly the same number 113. The general conclusions to be drawn from these results appear to be that the normal rate of pulsation is not reached unless there be at least a depth of about 130 to 170 millims. (say 6 inches), and that the effect of shallowness is as great on small as on larger amplitudes. No doubt the diminution in rate is due to the increased drag upon the bottom of the vessel. It is at about this depth that the to-and-fro motion of the particles at the bottom of the vessel ceases to be sensible.

But the examination of the effect of depth is not complete unless we know whether that effect is a function of the diameter or wave-length. Accordingly a circular vessel D was taken of the diameter 314 millims., with the following result:—

Depth of water.	Number of pulsations in 1 minute.	Mean n.
millim.		
150	149, 149, 149	149
130	149, 149, 148	148·7
120	by interpolation	147·9
110	147, 147, 147	147
100	by interpolation	146·9
90	145, 145, 145	145·7
80	143, 143, 143	143
70	142, 142, 142	142
60	139, 139, 139	139
50	134, 134, 134	134
40	126, 125, 126	125·7

It appears, then, that in the vessel D, whose diameter is 314 millims., the effect of the shallowness begins to be felt at rather lesser depths than is the case in the wider troughs. At depths of 30 millims. and under, the fundamental system cannot maintain itself.

§ 6. *Effect of variation in chemical nature.*—A cylindrical vessel, which I shall call D, had the diameter of 321 millims. It was filled to a depth of 80 millims. with methylated spirit. This gave in each of three experiments 143 pulsations per minute. Water of the same depth gave also exactly 143. We may conclude that, as in other oscillatory motions, the density of the

substance is without effect. This was shown experimentally, in the case of waves progressing in rectangular troughs, fifty years ago by the brothers Weber.

§ 7. *Effect of variation in the diameter.*—I used chiefly four cylindrical vessels. Their dimensions were as follows :—

Vessel A, mean diameter at surface of water 595 millims.

,,	B,	,,	,,	,,	456	,,
,,	C,	,,	,,	,,	366	,,
,,	E,	,,	,,	,,	300	,,

Experiments with A (depth = 290 millims.).

In 3′ there were 321 pulsations,

,,	5′	,,	535	,,
,,	4′	,,	427	,,
,,	10′	,,	1071	,,

or mean $A_n = 106.96$.

Experiments with B were given in § 3, whence it appeared that (depth = 290 millims.)

mean $B_n = 122.4$.

Experiments with C (depth = 285 millims.).

In 3′ there were 411 pulsations,

| ,, | 3′ | ,, | 410 | ,, |
| ,, | 3′ | ,, | 410 | ,, |

or mean $C_n = 136$.

Experiments with E (depth = 870 millims.).

In 1′ there were 151 pulsations.

| ,, | 1′ | ,, | 151 | ,, |
| ,, | 1′ | ,, | 151 | ,, |

or mean $E_n = 151$.

Putting the results together,

	Diameter.	Number of pulsations per 1′.
A . . .	595	106·96
B . . .	456	122·4
C . . .	366	136·73
E · . .	300	151

§ 8. In the binodal system of undulations here set up it is clear that the wave-length is equal to the diameter of the cylinder. If, then, the rate of progression is directly proportional to the square root of its length, we must have $v = C\sqrt{\text{diameter}}$, where C is a constant. But the length of path from the centre to the

circumference and back is the diameter; and the rate of pulsation (that is, the rate of recurrence of the same phase) must for uniform motion vary inversely with the path or diameter. Accordingly if n be the number of undulations in a time-unit,

$$n = C\,\frac{\sqrt{\text{diameter}}}{\text{diameter}},$$

$$\text{or } n\sqrt{d} = C.$$

We should get a constant on multiplying the square root of the diameter (or radius) of a circular trough with the number of pulsations per $1'$. In the following Table this is done. In column 1 are shown the values of $n\sqrt{d}$. For more palpable comparison, in column 2 are the numbers got by dividing each of column 1 by the least (which is the first).

				(1) $n\sqrt{d}$.	(2)
A	.	.	.	2608·035	1·0000
B	.	.	.	2613·747	1·0022
C	.	.	.	2615·799	1·0029
E	.	.	.	2615·397	1·0029

The close coincidence of these numbers establishes the law that the rate of wave-progression varies directly with the square root of the wave-length. The absolute velocities of progression of waves of the lengths established in the troughs are—

A	.	.	.	63·6412	metres per $1'$,
B	.	.	.	55·8144	,,
C	.	.	.	50·0437	,,
E	.	.	.	45·3000	,,

and of course these numbers, divided by the square roots of their respective wave-lengths, are constant.

According to A, a wave 1 metre long travels at the rate of 83·3060 metres in a minute.

According to B, at	82·6053	,,	,,
,,	C, ,,	82·7224	,,	,,
,,	E, ,,	83·6691	,,	,,

Or taking the mean, we may conclude that a wave a metre long would travel at the rate of 83·07 metres in $1'$ (a little over 3 miles an hour) if it expanded circularly, and moved freely and automatically without change of wave-length.

§ 9. *Form of fundamental binodal wave in cylindrical trough.*— The beautiful smoothness and persistence of the stationary binodal waves in cylindrical vessels enables us to examine their form

with some accuracy. It is at once seen that while the only part of the water free from radial motion is that in contact with the walls and the very axis of the cylinder, there is a nodal ring of constant height—which, however, is only a geometrical expression ; for the water sweeps to and fro through the node radially. Putting aside for the present the internal motion of the water, let us examine the position of the nodal ring. This can be done by shifting the gauge of § 2 until there is no rise and fall on its stem, and then measuring its distance from the edge. A permanent record of the nature of the wave-system can be got by immersing a sheet of cardboard in a vertical plane passing through the axis of the cylinder and reaching to the circumference. The cardboard loses its smoothness where the water has touched it. By both of these methods it appeared that in vessel A the node was 99 millims. from the edge, in B it was 75 millims., and in C it was 64 millims. The diameters of these being respectively 595, 456, and 366 millims., the fraction of the diameters at which the nodes were formed were respectively 6·01, 6·08, and 5·72. The central amplitude was in each case 26 millims. In the vessel C this amplitude causes incipient breakers, so that the nodal point sways. It appears that in a perfect system of such waves the nodal line is $\frac{1}{6}$ of the diameter from the circumference.

§ 10. *Relative amplitude at different parts.*—When the amplitude is great, the elevation at the centre exceeds the depression at the same place. The elevation may be so great as to project water repeatedly, while the corresponding cavity is smooth and round. In such a condition the node sways. With smaller amplitudes, the alternate depression and elevation at the centre are equal to one another, and the node becomes stationary.

With great amplitudes, that at the centre is indefinitely greater than that at the edge (when the node sways). With very small amplitudes the amplitude at the edge is nearly exactly one half of that in the centre ; this is the more nearly true the less the amplitude, and would, I conceive, be strictly true if the waves were conical instead of being dome-shaped.

§ 11. *Comparison with stationary waves of solids.*—A uniform elastic rod or lath, *a b c*, fig. 1, can be set in vibration in the manner shown by the dotted lines. It divides itself in such a way that $a=c=\frac{1}{2}b$ and $d=e=f$. The nodes are at $\frac{1}{4}l$ from the ends, and the amplitude at the ends is

Fig. 1.

nearly the same as in the middle. A lath consisting of two isosceles triangles (fig. 2) can be thrown into similar waves whose
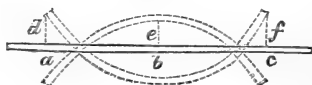
planes are at right
angles to the plane
of the lath (fig. 2).
But the nodes are
now at nearly $\frac{1}{6}l$
from the ends, and
the amplitude in

Fig. 2.

the middle is approximately twice that at the ends.

Imagine a thin circular board to consist entirely of such
sectorial laths.   They would, if strung on a circular wire passing
through their centres of gravity, form a rigid system ; for motion
around the wire would cause gaping between the apices and
crushing between the bases of the sectors.   Nevertheless such
motion is possible if the disk is elastic, as is shown by the for-
mation of a nodal line around a circular plate at a distance of
one sixth of the diameter from the circumference when the disk
is bowed through a hole in the middle.   The motion of such a
disk is analogous in many respects to that of water in a circular
trough.   We may compare both cases with systems of isosceles
triangles oscillating about lines passing through their centres of
gravity.   With the disk we have momentum and inertia con-
trolled by elasticity ; in the wave they are controlled by gravity.
If the surfaces of the waves were conical, the following simple
comparison might be drawn for an elementary displacement.

Let fig. 3, D E, be the
axis of the cylindrical
trough.   Let D G,
D B be the limiting
radii of the displaced
cone - sector D G B.
When D sinks to E,
G B will be at F H.

Fig. 3.
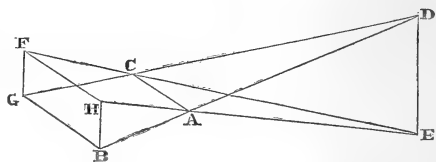
If the sector turns on the line C A parallel to its base and one
third B D from it, then D E = 2F G.   When D sinks to E, all
the water in the tetrahedron (A C D E) leaves it, and as much
water has to enter the wedge F G B A H C.   These volumes
must be equal.   It is easy to show that they are so.   This is in
accordance with the general law that when two heavy planes in
one plane turn together round any axis passing through their
common centre of gravity, they sweep out solids of equal
volume.

As the water is constrained neither to separate in the middle
nor from the walls, the sectors change from conical to circular
sectors and back again.   And with conical waves the actual
volume of water concerned in the motion is less than that traced
out by the rotating sector ; but this defect is the same in volume
with the wedge as with the tetrahedron.

§ 12. *Level of node.*—When the wave-system is perfect, the vertical height of the node is the same as that of the surface of the liquid when at rest. The movement of particles of powder on the water shows that though the node itself is at rest, the water composing it is in violent motion. It is a stationary market in a shifting population. It shows that, as in solid and gaseous waves, a node is a region where most work is done, and accordingly where least motion ensues—a hinge.

Rectangular Troughs.

§ 13. Four troughs of zincked iron were ordered of the common depth of 1 foot, the common width of 1 foot, and the respective lengths of 1 foot, 1 foot 6 inches, 2 feet, 2 feet 6 inches. They were found to have the following dimensions in millimetres :—

				Width.	Depth.	Length.
W	.	.	.	320	315	308
X	.	.	.	320	315	463
Y	.	.	.	315	315	619
Z	.	.	.	322	315	767

About the depth, my only care was to have such a depth of water that there should be no sensible drag by friction. This appears from § 5 to be the case with about 150 millims. of water. It will be shown in § 23 that, at this width, variation in width is without sensible effect. The mean widths are given. The lengths are also the means of twelve measurements 1 inch apart. In no case was there a difference of more than 1 millim. between lengths which should have been identical.

In rectangular troughs two chief wave - systems can be established which maintain themselves automatically. They may be called binodal and mononodal respectively. The first is the counterpart of the circular waves which we have been considering. The binodal system is got by placing a wooden lath, 2 or 3 inches wide and nearly as long as the trough is wide, and provided with a handle at right angles to its plane, on the surface of the water exactly halfway from either end, and depressing and raising it intact. The waves soon grow and maintain themselves for several minutes. The second or mononodal system is very easily produced by slightly tilting the trough repeatedly around one of its lower end edges. We shall consider the binodal motion first. The troughs were all filled to a depth of 260 millims. As it appears from the whole of the subjoined experiments that the number of undulations in the first minute was the same as those in the second, and so on, it followed that, as in circular troughs, the amplitude was without effect upon the frequency of pulsation.

§ 14. *Variation in length.   Experiments with* W.

In 1′ there were 136 pulsations.
   „ 1′   „   135·5   „
   „ 1′   „   136   „

Mean number of pulsations in 1′ for W . . . 135·8.

In this square trough it is difficult to avoid the travelling of the nodal lines till they become diagonals.

Experiments with X.

In 1′ there were 111 pulsations.
   „ 1′   „   110   „
   „ 1′   „   110   „
   „ 2′   „   · 221   „

Mean number of pulsations in 1′ for X . . . 110·4.

Experiments with Y.

In 2′ there were 189 pulsations.
   „ 2′   „   189   „
   „ 2′   „   189   „

Mean number of pulsations in 1′ for Y . . . 94·5.

Experiments with Z.

In 2′ there were 169·5 pulsations.
   „ 3′   „   253·5   „
   „ 4′   „   338   „

Mean number of pulsations in 1′ for Z . . . 84·5.

Before collating these results we must examine the effect of shallowness.

§ 15. *Effect of depth on binodal undulations in rectangular troughs.*

Depth.	Number of undulations per 1′ in			
	Z.	Y.	X.	W.
millim.				
150	......	......	......	133
170	......	......	......	134
190	......	......	°	135·8
210	82	92	109	135·6
220	83	93	110	135·4
240	84	94	110·4	135·6
260	84·5	94·5	110·4	135·8
280	84·5	94·5	110·4	136
300	84·5	94·5	110·4	135·6

From this Table it appears that in rectangular troughs of the same width and different lengths, a diminution in the depth of

the water takes effect in increasing the period on the longer trough before it affects the shorter one. In other words, to exhibit its normal (*i. e.* maximum) rate, a short trough need not be so deep as a longer one.

It also appears that the depth 260 millims. employed in the experiments in § 14 is sufficient.

We may therefore compare the results of § 14.

		Length.	Number of pulsations.
Z	. . .	767	84·5
Y	. . .	619	94·5
X	. . .	463	110·4
W	. . .	308	135·8

The wave-length is here, of course, the length of the trough. If the rate of progression varies directly as the square root of the wave-length, then, since the rate of repetition of phase varies inversely as the path, which is also the trough-length, we should get (as in § 8) a constant on multiplying the number of pulsations in a minute with the square root of the trough-length. This is done in the following Table, where also the quotients got by dividing all by the smallest are given :—

		$n\sqrt{l}.$	
Z	. . .	2340·21	1·0000
Y	. . .	2341·14	1·0004
X	. . .	2375·52	1·0108
W	. . .	2383·29	1·0183

These numbers are even more nearly in accord than those of circular troughs of § 8. They show that the rate of progression of a restrained wave in a trough varies also directly as the square root of its length. But the question asks itself, How is it that $n\sqrt{l}$ of straight troughs is uniformly less than $n\sqrt{d}$ of circular ones? The mean value of the constant in the circular system is 2618·652; in the rectangular it is 2360·04. These are in the ratio of 1·109 to 1. To this point I shall return in § 23.

The absolute velocities of progression of the waves are :—

Z	. . .	64·8115	metres in 1′.
Y	. . .	58·4955	,,
X	. . .	51·1152	,,
W	. . .	41·8264	,,

If we assume the formula

$$v_1 = v_2 \sqrt{\frac{l_1}{l_2}}$$

to be true for the wave-length of a metre (and we see it is approximately true for 767 millims.), it is found that a wave a metre long in a rectangular trough travels,

According to Z, at the rate of 74·006 metres in 1′.

,,	Y,	,,	74·348	,,
,,	X,	,,	75·088	,,
,,	W	,,	75·371	,,

The mean of these is 74·7, and this is the distance which a wave a metre long would pass over in 1′ when moving in deep water between parallel walls. This is under the assumption that no velocity is lost by reflection; and the same assumption must be made in § 8 with circular troughs, where the velocity of the metre wave in circular troughs was found to be 83·1 metres in 1′.

§ 16. As the circular undulation was referred to an oscillating triangle or vibrating pair of triangular laths, so may the motion of the water above described in the rectangular trough be likened to the balancing of two rectangular laths supported at their lines of gravity, or, more closely, to the binodal vibration of a rectangular lath (see § 11). The lines of gravity are now from each end $\frac{1}{4}$ of the joint length. The nodes are at $\frac{1}{4}l$ from each end. Accordingly in rectangular troughs the nodes are at exactly $\frac{1}{4}l$ from the ends when the undulation is binodal. Also, on taking a paper section of the system (see § 9), it is found that the amplitude at the centre only exceeds that at the ends by a very small amount. As before, the node is not a line of material rest. The water sweeps through it towards the mountain which is forming at one side or the other.

§ 17. *Mononodal undulation in circular troughs.*—In circular troughs the mononodal waves can be produced by tilting or by a segmental stirrer. The former, of course, lifts the node and produces at first a longer wave than is due to the proper limits of the trough. This very fact causes the wave to accommodate itself to the trough, and to establish a nodal line which is a diameter of the cylinder at right angles to the plane of the wave. The same system is produced at once by the careful use of the segmental stirrer. Two kinds of irregularities may show themselves. The one depends upon the partial degeneration of the mononodal system into circular bi- or quadrinodal systems; the second consists of an almost unavoidable rotation of the node. Such rotation, which need not exceed a quadrant in three or four hundred waves, must of course be an experimental fault, since no reason can be assigned why it should take place in one rather than in the other direction. Neither irregularity appears to affect the rate of the mononodal system; the first gradually effaces itself probably by disaccord between the older and newer effects.

§ 18. *Effect of depth on the mononodal system in circular troughs.*

Depth.	Number of undulations per 1′ in	
	A.	B.
millims.		
145	63	76
155	......	78
165	......	79
175	65·5	80
195	......	81
225	......	82
255	71	83
280	71·5	83·5
330	71·6	83·5

§ 19. *Effect of variation in diameter on the above experiments with* A (depth = 290 millims.).

> In 2′ there were 143 waves.
> „ 2′ „ 143 „
> „ 2′ „ 143 „
> Mean rate for A per 1′ . . 71·5.

The second and third 2′ were consecutive, and accordingly the wave-rate of sequence is independent of amplitude.

Experiments with B (depth = 290 millims.).

> In 2′ there were 167 waves.
> „ 2′ „ 167 „
> Mean rate for B per 1′ . . 83·5

Experiments with C (depth = 185 millims.).

> In 2′ there were 183 waves.
> „ 2′ „ 183 „
> Mean rate for C per 1′ . . 91·5.

Experiments with E (depth = 870 millims.).

> In 1′ there were 103·5 waves.
> „ 1′ „ 103·5 „
> „ 1′ „ 103·5 „
> Mean rate for E per 1′ . . 103·5.

Putting the results together—

	Diameter.	Mean n.	$n \sqrt{d}$.	Depth.
				millim.
A	595	71·5	1739·09	290
A′	595	71·6	1741·53	330
B	456	83·5	1782·06	290
C	366	91·5	1760·49	185
E	300	103·5	1789·62	870

It appears both from § 18, and from the above, that A or even A′ was scarcely deep enough to give the normal rate for their great wave-lengths. It also appears that for the shorter system of C the depth 185 is insufficient. From § 18 it seems that the maximum or normal rate is reached. We may take the nearly identical numbers for B and E for the valuation of the mean constant, which is 1785·84 (in metre-minute measurement).

[To be continued.]

XXXVI. Mr. Mallet's *Theory of Volcanic Energy tested.*
By the Rev. O. FISHER, *F.G.S.* *

IN the July Number of this Magazine Mr. Mallet has replied to certain remarks upon his theory of volcanic energy made by Professor Hilgard† and by myself‡, and containing something in common. Professor Hilgard suggested an objection to the theory which he thinks "must strike every reader of the original memoir," and assumes not to require proof—that "the maximum temperature resulting from the crushing to powder of the hardest rock is something over 217° Fahr." as determined by Mr. Mallet. But inasmuch as Mr. Mallet in his memoir appeared to rely upon the localization of the heat produced in crushing the *whole* of a certain quantity of rock for fusing *one* part of it, I attempted to show that such localization of the heat was not possible, and that consequently, if crushing rock could fuse it, *all* the rock so crushed ought to be fused; and, inasmuch as for any thing that appeared to the contrary, Mr. Mallet relied upon the result of his experiments for giving the full amount of heat obtainable in that way, I concluded that his argument required that the cubes experimented upon ought to have been themselves fused.

In his reply Mr. Mallet commences by suggesting that the heat evolved by crushing rock in the depths of the earth's crust may be much greater than what he inferred by calculation from his experiments; for it must be borne in mind that the heat was not measured, but calculated. However, I will begin by commenting on his answer to my first objection (which he has taken second), viz. that the heat produced by crushing could not be localized; for that it could be so is obviously essential to the argument of the original memoir, in which he shows that

* Communicated by the Author.
† American Journal of Science, 3rd Series, vol. vii. no. 42, Art. L. p. 535; Phil. Mag. for July 1874, p. 45.
‡ Quart. Journ. Geol. Soc. vol. xxxi. p. 469.

the heat calculated to be evolved out of crushing one volume would fuse about one tenth of an equal volume *.

Let us, then, consider a prism of rock of any length. This is itself a part of the earth's crust, and by its resistance has, up to the moment of its giving way, resisted, and so permitted, the necessary accumulation of the pressure which eventually causes it to yield. Conceive that in this prism there are portions situated here and there which are weaker than the average, and that these weak portions when crushed allow of the prism being shortened at the places where they are situated by the quantities α_1, α_2, α_3, &c. respectively. It is clear that the weaker places will yield first and under a less pressure, and by the relief so afforded delay the crushing of the others, because the pressure must accumulate afresh. But, for argument's sake, we will suppose all to yield together, and the pressure throughout the action to be equal to the value it had at the first yielding.

Now suppose that when the prism has yielded the whole of it becomes shortened by the length a. If, then, P be the pressure which caused it to yield, Pa will be the whole work done upon the prism. The length a is made up of the portions α_1, α_2, α_3, &c., by which the weak portions have been shortened; while Pα_1, Pα_2, Pα_3, &c. are the portions of work done at these places. And these taken together make up Pa, since $\alpha_1 + \alpha_2 + \alpha_3 +$ &c. $= a$. We see, then, that the work must be confined to these places; for if there were work done elsewhere we should have more work than Pa, which is impossible. Hence the work convertible partially into heat takes place at all these places, and at each in proportion only to the yielding, and nowhere else; so that it cannot be localized at any one place. It seems to me, therefore, that, unless the heat got out of crushing any portion of rock is sufficient to fuse that particular portion, none will be fused; and this led me to my second objection (put first by Mr. Mallet), that, if crushing rock could fuse it, the cubes experimented upon ought to have been fused; for I certainly understood Mr. Mallet to rely upon his experiments for showing how much heat might be obtained by that means.

The supposed weak parts of the prism answer to the joints of ashlar stones, as instanced by Mr. Mallet in his late paper in this Magazine.

In reply to my objection last mentioned, Mr. Mallet enters upon the consideration of the circumstances under which crushing may be thought to occur deep down in the earth's crust, and holds

* § 188. "It follows from equation (6) that 1 cubic mile of crushed mean rock will thus heat 0·262 cubic mile of the heated material, or will fuse 0·108 cubic mile of the fused material, the specific heat of all being taken as the same."

that these are fully sufficient to refute it. He says, "indeed the statement that if under any circumstances and in the rock-masses of nature 'crushing can induce fusion, then the cubes experimented upon ought to have been fused in the crushing,' seems as unsupportable as it would be to affirm that no heat is developed by the slow oxidation (*eremacausis*) into water and carbonic acid of a pound of wood, which when burned develops a well-known amount of heat." I may observe that I made no statement about the "rock-masses of nature," but simply commented on Mr. Mallet's arguments, such as I understood them to be; for I did not see the bearing of the numerous and elaborate experiments, and laborious calculations founded upon them, unless it was held by him that the results as he had left them were held by him to be applicable to the case of nature. I certainly supposed that such was the author's meaning; and it seems that Professor Hilgard came to the same conclusion. I am also at a loss to perceive how the instance of eremacausis bears upon the question. If it represents the cubes crushed in air, and the quick burning represents the rock crushed low down within the crust, then it would support the view that the heat developed in both cases is the same; for the total amount of heat developed by the burning of the wood is the same in either case, though the times taken to develop it differ. In Mr. Mallet's experiments time does not enter as a factor; for the work is measured by *pressure × distance through which it acts,* and no deduction is made on account of any escape of heat.

We will now follow Mr. Mallet into his developments of the original theory, as given in his paper "On the Temperature attainable by Rock-crushing and its Consequences."

"If a cube of rock, which in free air is found to crush under a certain pressure, be situated deep within a mass of similar rock and there crushed, it does not admit of dispute that the work necessary to effect crushing must be largely increased."

This does not appear easily reconcilable with the statement of § 92 of the original memoir. But, admitting it to be the case, and that the pressure required to crush cubes at the depths of 10 and 20 statute miles will be as supposed 2·14 and 4·28 times as great as in air, it of course follows that "if we assume the displacement of the crushed particles after crushing to be the same as in the case of the cube crushed in air, then the work and the heat due to its transformation will be also 2·14 or 4·28 times as great." But it seems impossible that a cube of rock so situated could be reduced to a cake of considerably increased diameter, as were the cubes experimented on; and therefore if one factor in the expression for the work should be increased, as Mr. Mallet believes, the other must be at the same time greatly

diminished, because the four lateral faces of the cube cannot
have the same liberty of distention as in the experiments. We
cannot, therefore, feel assured that the heat developed will increase
in proportion to the superincumbent pressure. If the cube in
the experiment had been just crushed in a close-fitting box, and
the heat developed measured thermometrically, the proportionality
might have been more reliable. It is I think to be regretted
that Mr. Mallet has not given the thermometric results of his
experiments in § 99, although even in them the box in which
the rock was enclosed did not touch it round the periphery, and
so was far from representing the case of nature.

But in truth the principal arguments in the paper under con-
sideration have regard to Professor Hilgard's suggestions as to
the *"combined influence of friction and rock-crushing."* And it
is really most important to examine the question under this
extension; for that Mr. Mallet's sagacity has pointed out a
subterraneous source of heat which had been overlooked is
undeniable, while at the same time the question whether it
will account for volcanic action is, I presume, not yet settled.
He now suggests the possibility of the rock, after being crushed,
being pushed aside, leaving part of its heat behind it, and the
rest of the column being crushed down upon the same spot of
a hard rock wall, and taking up some of the heat left from the
antecedent portion in addition to what is proper to its own
crushing.

To judge of what must happen under the circumstances, we
must consider what the supposed column is. It is in truth a
horizontal element of a vertical slice of the solid crust. Mr.
Mallet argues thus respecting it :—" The first cubic foot of the
column that is crushed has its temperature raised, let us suppose,
by the minimum 217°. The crushed fragments at this tempera-
ture are pushed aside by the advancing column, whose extremity
is thus surrounded by crushed material at a temperature of 217°,
and the second foot in length of the column becomes crushed.
But the material of this second cubic foot is at a higher tem-
perature before it is crushed than was the first cubic foot; so
that the heat due to the transforming work of crushing of each
successive cubic foot of rock raises its temperature to a higher
point than that of the preceding one, because each successive
cubic foot at the instant before crushing is at a temperature
already higher than the preceding ones, resulting from the heat
taken up by the uncrushed column from the hotter portions of
material surrounding it that have already been heated by the
crushing."

Now it seems that the above reasoning might possibly be
applicable to an isolated column, crushed down upon a hard

plane, if the column were originally weakest at the point of contact and the parts nearer that always weaker than those more remote, it being also provided that it did not bend or "buckle" anywhere.   But the column must in any case be *isolated*; else the supposed "pushing aside" and surrounding of the end of the column by the "hotter portions" could not take place.   It seems to me that we here get very far away from the condition of the "rock-masses of nature."   If there be any pushing aside of material, it seems that it must happen to the whole vertical thickness affected, and must manifest itself by extrusion at the surface of matter not melted; for the temperature is admittedly not high enough for that in the portions first crushed.

Looking at the matter thus, it seems that, if fusion of the matter of the earth's crust can be induced by the mechanical consequences of gravitation, it must be, as Professor Hilgard suggests, by the *friction* of rock-masses amongst themselves, but that surface-movements commensurable with the internal friction must in that case manifest themselves.   With this aspect of the question Mr. Mallet has not dealt ; and, as Prof. Hilgard observes, " of the complex thermal effects of the movements of detrital masses under great pressure, his figures offer no measure whatsoever."

Nevertheless on the main question Prof. Hilgard thinks that " Mallet's experiments on the contraction of fused rock in cooling, and his estimates of the amount of volcanic energy manifested on the globe, coupled with that of the earth's annual loss of heat, completed the proof of the *quantitative* adequacy of the cause invoked by him."   And he adds, "the burden of proof of the *qualitative* inefficiency of the several modes of action that may come into play, would seem to be effectually thrown upon the opponents of the theory."

In connexion with this branch of the subject it is important to refer to a paper by Mr. Mallet in the Philosophical Transactions, entitled an "Addition to the Paper on Volcanic Energy "*.   In this he has made a comparison between his estimate of volcanic energy and the particular store of energy from which he supposes it derived, namely the crushing of the rocky matter of the crust.   Upon the basis of Sir W. Thomson's estimate, that the heat annually lost by the earth would melt 777 cubic miles of ice at $32°$ F., he estimates the annual descent of the shell on suppositions of its being 100, 200, 400, 800 miles thick respectively.   The estimates are founded upon the supposition that the amount of heat so lost is derived from the

* Phil. Trans. vol. clxv. pt. 1.

cooling of the *nucleus*, from which it passes through the crust. Consequently he assumes for the coefficient of contraction a mean of those higher values which he has shown experimentally to obtain for exalted temperatures. In passing, it may be remarked that this seems to be opposed to Sir W. Thomson's conclusions in his paper "On the Secular Cooling of the Earth"*, in which his diagram shows that, if no convection-currents exist within the nucleus, the heat in the deep interior of the earth has scarcely decreased at all since it was all melted, but that the loss has taken place chiefly from the outer portions, and continues to do so still. Hence it is doubtful whether Mr. Mallet's mode of estimating the contraction is admissible, and whether he has not consequently made the annual descent of the surface too great.

Having in this manner estimated the annual descent of the shell, he calculates the amount of volume by which the shell will, after its descent, be found too large to fit the contracted nucleus; and he supposes that the superabundant matter is crushed and extruded, giving forth the amount of heat appropriate to its volume, as estimated from the experiments discussed in his original paper. He then compares this amount with the amount of heat estimated to be sufficient to maintain volcanic energy for a year, and finds it somewhat greater than sufficient if the shell be 400 miles thick.

Now it is obvious that this mode of arguing assumes the numerical correctness of the experimental results and their applicability to the case of deeply buried rocks. But it seems that the whole subject will be placed on a more intelligible basis if some comparisons be made which may be independent of this particular assumption.

With a crust 400 miles thick Mr. Mallet reckons that the annual radial contraction would be 0·00000000928 mile, or 0·00058995 inch.

We will then proceed to find the number of units of heat due to the annual descent of the crust on these suppositions :—

R, r the external and internal radii of the crust,
d its descent,
ρ its density.

Then the work of descent is approximately equal to

$$4\pi\rho g R^2 (R - r)\, d.$$

$$R = 20,890,000 \text{ feet, say } 21 \times 10^6 \text{ (about 4000 miles)},$$

$$R - r = 400 \text{ miles} = \frac{R}{10}$$

$$= 21 \times 10^5 \text{ feet},$$

$$g = 32 \text{ feet},$$

$$\rho = 2 \cdot 6,$$

$$d = 0 \cdot 0006 = \frac{6}{10^4} \text{ inch}$$

$$= \frac{5}{10^5} \text{ foot}.$$

Substituting the above values, we get for the work of descent of the crust 4851×10^{16}; and dividing this by Joule's equivalent (772), we obtain for the corresponding number of units of heat

$$6283 \times 10^{13}.$$

It does not appear that Mr. Mallet has directly expressed the annual amount of volcanic energy in terms of units of heat; but he considers that he "has shown that its total amount cannot exceed a small fraction of the entire heat dissipated annually, being only $\frac{1}{1589}$ thereof"*. We have therefore first to express in units the heat annually dissipated.

Now 777 cubic miles $= 1144 \times 10^{11}$ cubic feet (about). And each cubic foot of ice requires 8265 units of heat to melt it (Addition, p. 4), or say 8×10^3. Hence the number of units of heat annually lost by the globe is

$$1144 \times 10^{11} \times 8 \times 10^3 = 9152 \times 10^{14}.$$

And it has been just shown that the entire work of subsidence of a crust 400 miles thick is equivalent to

$$628 \times 10^{14} \text{ units of heat.}$$

The latter is therefore $\frac{628}{9152}$ of the former, or about $\frac{1}{15}$.

Mr. Mallet reckons the annual volcanic energy at $\frac{1}{1589}$ of the whole heat dissipated; whence we obtain approximately,

Volcanic energy : the entire energy of subsidence : : 1 : 100. So that, if the nature of the materials by their resistance to motion could allow P (the horizontal pressure) to accumulate to a sufficiently large value, and if no work were done upon the nucleus by the descent of the crust, there is no doubt that there would be enough and to spare in the annual subsidence to account for the required amount of energy as he has estimated it. Much would depend, however, upon the degree of such resistance. For instance, if the rocks were fluid there would

* Addition to the paper on Volcanic Energy, par. 1, Phil. Trans. vol. clxv. pt. 1.

be no resistance, and if absolutely rigid no movement, so that in either of these extreme cases no heat would be developed in the crust.

It is easy to see that the whole work which can be obtained out of the lateral compression is the same thing as the whole work of subsidence; for let ds, ds' be the edges of a rectangular element of the shell, k its thickness, e the coefficient of compression, which is evidently the same as that of radial contraction under the suppositions made above, P the mean pressure on a unit of area of a vertical section of the crust; then P k ds is the pressure on a face of the element.

And P$kds \times eds'$ is the work of compression of that element in the direction perpendicular to ds.

Similarly, P$kds' \times eds$ is the work in the orthogonal direction. And the sum of these will be the whole work on the element.

Hence the entire work on the crust will be

$$2\mathrm{P}ke \iint ds\, ds'.$$

Or work of lateral compression $= 8\mathrm{P}ke\, \pi \mathrm{R}^2$.

Now, if we give P its full value, viz. $g\rho\dfrac{\mathrm{R}}{2}$*, we get

$$\text{work} = 4\,\pi\rho g\, e\mathrm{R}^3 k.$$

Comparing this expression with that for the whole work of descent of the shell, and observing that $e\mathrm{R} = d$ and $k = \mathrm{R} - r$, we perceive the two to be identical.

In nature, however, P is not likely to be ever so great as this. But the fact that the *utmost* annual amount of energy which can be got out of subsidence of a crust 400 miles thick on a very favourable supposition is only one fifteenth of the energy dissipated from the globe, shows how large a store of heat there must be within the nucleus.

We will suppose, then, that the rocks are of such a character, and under such conditions, that an amount of heat sufficient, and more than sufficient, to account " *quantitatively*" for all volcanic phenomena is capable of being developed.

I accept Professor Hilgard's challenge to prove its "*qualitative*" insufficiency, by which I understand that it cannot be localized under such conditions as to produce the volcanic phenomena wn to occur.

Let us thus then survey the conditions of the problem. A contracting globe induces in its enveloping crust a state of compression, which owes its existence to the gravitation of the whole

* See the author's paper " On the Elevation of Mountains by Lateral Pressure," Trans. Camb. Phil. Soc. vol. xi. pt. iii. p. 492 (1868). Also Mr. Mallet on Volcanic Energy, Phil. Trans. 1873, p. 173, § 83.

towards the centre of figure. The interior goes on contracting until the compression accumulates sufficiently to cause a' movement of some kind among the particles of the crust, be it crushing, or faulting, or corrugation, or what not; and the motion being arrested, the work becomes transformed into heat. This heat, as I have already proved, can be developed only at those places where the movement occurs. The question is whether it will be sufficient to give rise to a volcano. If some places are weaker than others, the movement will necessarily occur at them. Let there be several such, which for simplicity we will suppose to be situated at points upon a great circle. The force of compression will go on increasing until one of these places gives way. Let P_X be a general symbol for this increasing force, measured by the pressure, in terms of the weight of a cubic mile of mean rock at the surface, upon a square mile of vertical section of the crust.

Now it is evident that no greater lateral shortening, or approach of the particles of the crust among themselves, can take place at one place than at another, except through a lateral movement of the layers of the crust over the nucleus towards the place in question. Hence any localization of work at a given place must require this to occur more or less. Consequently if the pressure goes on accumulating until it becomes equal to P_X all round, and under that pressure the crust begins to yield at A, the yielding there cannot relieve the pressure anywhere else, unless the crust is shifted over the nucleus towards A.

Having premised thus much, we will endeavour to find the amount of heat which can be developed along a vertical section at any one place in the crust under several conditions.

Conceive a strip of the crust, one mile in width, situated along a great circle; and suppose the pressure to have gone on increasing to some value P_X, which has caused the crust to yield

at A; and that by such yielding the pressure has been reduced to P_A at A, and relieved over A B and A B' on either side of A, but not beyond B and B'. Hence at B, B' the pressure will still be P_X. Beyond B and B' it will have been relieved by yielding at other places; and it is supposed that it does so yield all round, so as to fit the reduced nucleus. Hence the amounts to which the pressure will accumulate at different places will depend upon the strength of the crust; but the amount of *shortening* of the whole crust will not depend upon the amount of the pressure, but solely upon the amount of contraction of the nucleus. Consequently if e be the coefficient of linear or radial

contraction, seeing that the points B, B′ are not shifted upon the nucleus, it follows that the compression of B B′ is e B B′.

Let μ be the force which a unit in length of the crust would just resist, so as under its action not to move over the nucleus. Then, before motion commenced, μ would depend on the adhesion—but after motion had commenced, on friction; and μAB is the force which the length AB would just resist. Let, as before, k be the thickness of the crust. In general μ will not be the same for BA and B′A; but we will suppose it so, in which case BA and B′A will be equal. Hence, after compression has taken place, we shall have for equilibrium, at sections passing through B and B′,

$$P_X k = \mu AB + P_A k,$$

and

$$P_X k = \mu AB' + P_A k.$$

Adding

$$2P_X k = \mu BB' + 2P_A k,$$

whence

$$BB' = 2 \frac{P_X - P_A}{\mu} k.$$

Therefore the compression between B and B′, which is supposed to be localized at A, is eBB′, or

$$\text{compression at A} = 2 \frac{P_X - P_A}{\mu} ek.$$

Respecting the force which has acted at A to give rise to this compression, we observe that it was $P_X k$ when the compression began, and $P_A k$ when it ceased. We may therefore put it at their mean, or $\frac{P_X + P_A}{2} k$. Hence the work at A, which is the product of these two quantities, $= \frac{P_X^2 - P_A^2}{\mu} k^2 e$.

Now our first object shall be to find a limit which must exceed the greatest value that the above can reach. It is evident that this will be given by assigning to P_X the utmost value of the compressing force, viz. P, and giving to P_A the value zero. It will be recollected that P is the weight of a column of rock 2000 miles high and 1 mile in sectional area, and of the density of the crust.

A superior limit to the whole work on a vertical section of the strip of crust will therefore be

$$\frac{P^2}{\mu} k^2 e;$$

and upon a square mile of this section,

$$\frac{\mathrm{P}^2}{\mu}\,ke.$$

Mr. Mallet, at p. 8 of his paper in this Magazine for July, states that friction may be as great as $\frac{3}{4}$ of the pressure. Let us, then, suppose first that the nucleus is solid, and that the force requisite to move a column of the crust horizontally over the nucleus is $\frac{3}{4}$ of the weight of the column, and P is the weight of a column 2000 miles long. Hence

$$\mu = \frac{3}{4}\,\mathrm{P}\,\frac{k}{2000}\,;$$

whence, if $k = 400$ miles,

$$\mu = \frac{3}{4}\,\frac{\mathrm{P}}{5}.$$

Hence the work upon a square mile of section cannot be so great as

$$\tfrac{4}{3}\mathrm{P}5ke,$$

which

$$= \tfrac{4}{3}\mathrm{P}e \times (2000 \text{ miles}).$$

Now in order to calculate the heat by means of Joule's equivalent, we require to have the force expressed in pounds and the space in feet. Let f be the number of feet in a mile.

The weight of a cubic foot of granite, as given by Mr. Mallet, is 178·3392 pounds (say 200 pounds). Hence the weight of a cubic mile of such rock will be $f^3 \times 200$ lbs.; and of a column 2000 miles high, or P, $f^3 200 \times 2000$ lbs.;

$$\therefore \mathrm{P} = f^3 \times 4 \times 10^5 \text{ lbs.}$$

The coefficient of compression for one year will be the same as that of the radial contraction, which, on the data assumed by Mr. Mallet (Addition &c., Table II.), will be, for 400 miles of crust, $\dfrac{9}{10^9}\dfrac{1}{4000}$;

$$\therefore e = \frac{9}{4}\frac{1}{10^{12}}.$$

We have therefore, expressing the space in feet,

Work in foot-pounds per square mile of vertical section at **A**

$$= \frac{4}{3}f^3 \times 4 \times 10^5 \times \frac{9}{4} \times \frac{1}{10^{12}} \times 2000 \times f.$$

And in foot-pounds per square foot, dividing by f^2,

$$work = \frac{4}{3}\, 2f^2\, \frac{9}{10^4}$$

$$= 76908 \, ;$$

$\therefore$ *Heat per square foot* $= \dfrac{work}{772} = 99$ units.

The corresponding value for BB′ will be 5333 miles.

Now, according to Mr. Mallet's results in his original memoir, § 133, (1) and (6), we are informed that 6472 units of heat would fuse 0·108 cubic foot of mean rock previously at 300° F., taking 2000° as the melting temperature. Hence with a solid earth, according to our calculation, *a limit greater than the greatest possible* gives 0·0015 cubic foot of mean rock that could be fused annually for each square foot of the plane of vertical section. With this value it would take a thousand years to fuse *one* vertical slice of a foot and a half thickness within the range of crust defined by the distance BB′, or 5333 miles, on a great circle of the sphere. But the place of weakness cannot but have a considerable thickness throughout which the heat would be distributed. If, then, it were, say, a mile thick, the number of units of heat to each cubic foot would be $\dfrac{99}{5280}$, or about 0·02 unit.

The force of compression, besides the work at A where the yielding takes place, also does work over the plane of shearing in overcoming adhesion and friction. Let the whole force expended in overcoming adhesion and friction along BB′ be μ'BB′. The space moved over increases from nothing at B and B′ to eAB and eAB′ at A. The mean is $\dfrac{\mathrm{BB'}}{2}$. Hence the work along this plane is

$$\mu'\mathrm{BB'} \times e\, \frac{\mathrm{BB'}}{2}$$

$$= 2\mu'\frac{\mathrm{P_x - P_A}}{\mu}\, k \times e\frac{\mathrm{P_x - P_A}}{\mu}\, k.$$

And making the same suppositions as before for a superior limit, this becomes on the whole plane

$$2u'\frac{\mathrm{P}^2}{\mu}\, ek.$$

We took μ at the value $\frac{3}{4}$ of the weight for friction. We will take it as equal to the weight for the adhesion, since that is probably greater than the friction, although its ratio to the fric-

tion probably diminishes as the pressure increases.   Hence

$$\mu' = \tfrac{4}{3}\mu \,;$$
$$\therefore \text{ the work} = \frac{8}{3}\frac{P^2}{\mu}\,ek^2.$$

But the whole work on a vertical section was found to be $\dfrac{P^2}{\mu}ek^2$; hence the whole work along the plane of shearing is $2\tfrac{2}{3}$ of the work along the vertical section.   This work will not be evenly distributed, but will be greatest upon the unit of the length nearest A, where the space moved over will be eAB, and the force upon it is μ'.   Hence work upon the last unit $=\mu'eAB$,

$$= \mu'e\frac{P_X - P_A}{\mu}\,;$$

upon the suppositions made, this becomes

$$= \tfrac{4}{3}Pek.$$

And the work upon a square mile of section on the same suppositions was found to be

$$\tfrac{4}{3}P5ek.$$

Hence the work, and therefore the heat, upon the last unit of the plane of shearing, where it will be greatest, is only one fifth of the work and heat respectively on a unit of the vertical section.

This result seems unfavourable to the view that the heat arising from friction of the rocks along horizontal planes can account for the heat concerned in producing even metamorphism.

It must be fully understood that the results just arrived at with respect to the heat developed at a vertical section, as well as along the plane of shearing, have reference not to any *probable* values, but to a *superior limit* which must be greater than the greatest possible.   In fact they involve several impossible assumptions, all of which make it too large.   These are:—

(1) That the contraction from cooling takes place in the matter of the nucleus and not in the crust.

(2) That the above circumstance allows the coefficient of contraction to be nearly four times as large as it would be on the more probable supposition.

(3) That the pressure increases to the utmost attainable value, viz. that of a column of rock 2000 miles long, before yielding takes place.

(4) That when yielding does take place, the pressure at the place of yielding entirely vanishes.

The above assumptions, besides making the amount of heat impossibly great, also localize it to an impossible degree, as may be seen by observing that BB' would be lessened by diminish-

ing P_X, and also by increasing P_A; that is, by lessening the pressure which causes the yielding, and by increasing the pressure at the place of yielding after the compression has taken place.

Mr. Mallet has obtained experimentally the pressure necessary to crush small cubes of rock of various kinds. These are valuable results as far as they go; but they do not tell us much of the pressures which would be required to cause the strata of the earth to yield at great depths. In his paper of July, p. 4, he makes some estimates upon this point, which, for the sake of illustration, we will adopt.

Let us then take 434×10^4 lbs. as the force per square foot which just causes a cube of rock to yield. Mr. Mallet thinks that 2·14 times this will be the pressure requisite at 10 miles depth and 4·28 times at 20 miles. Call the height in feet of a column of rock which would just crush the cube p. A cube of 1 foot of such rock weighs, say 200 lbs. Hence, if p be the height which will just crush,

$$p \times 200 = 434 \times 10^4 ;$$

$$\therefore p = 217 \times 10^2 \text{ feet.}$$

The pressure at 400 miles depth will therefore be that due to a column of the height

$$40 \times 2\text{·}14 \times 217 \times 10^2 \text{ feet} ;$$

and dividing by the number of feet in a mile (5280), this gives for the compressing force necessary to crush the section of the crust throughout, $P_X = 35$ miles of rock.

It is obvious that this force is quite inadequate with the assumed value of μ to shear the crust over the nucleus. We must therefore suppose P_X to accumulate to a much greater degree.

Let us, then, take it just sufficient to shear the crust, which it has been seen is probably the pressure due to the thickness of crust. It is clear that this compressing force will not be confined to one depth alone, since it is supposed to arise from the contraction of the nucleus and not of the crust. Hence suppose $P_X = 400$. But after the crust has been compressed, it is reasonable to suppose that there will be still some pressure at A which cannot well be less than the hydrostatic pressure due to the depth; call it, therefore, the mean of this for the whole depth, or $P_A = 200$. Hence we have

$$P_X{}^2 - P_A{}^2 = 160000 - 40000 = 120000.$$

Hence we have

$$\frac{\textit{work in the supposed case}}{\textit{work in limiting case}} = \frac{12 \times 10^4}{2 \times 10^6} = 0\text{·}06.$$

And the heat will be in the same proportion, viz. :—

Heat per square foot of vertical plane section of crust

$$= 0.06 \times 100 \text{ units}$$

$$= 6 \text{ units per annum.}$$

In the case above supposed,

$$BB' = 2\,\frac{400 - 200}{\mu}\,k$$

$$= \frac{400}{\frac{3}{4}\,\frac{P}{5}}$$

$$= 1\tfrac{1}{3} \text{ mile} ;$$

so that the heat would scarcely be localized at all.

In presence of the enormous pressures to which the interior of the earth must be subject, if they be no greater than would arise from fluid pressure alone, a compressing force due to a column of 400 miles of crust at a like depth is very probably considerably smaller than the true value, which, within the extreme limit of about 2000 miles pressure, is determined solely by the pressure the strata can sustain. It is extremely probable, therefore, that the heat per unit of vertical section might be considerably above 6 units per annum. But even then we get but a very small way towards what may reasonably be thought requisite to supply volcanic energy, especially when we consider that it must be distributed through the whole thickness of the weak section.

The above estimates have been made upon the supposition that the action between the crust and the nucleus is of the nature of friction between solids. If, however, as is more likely, the crust rests upon a fluid or viscous layer, the resistance to lateral motion will be much smaller; but we are not able to guess what it will be; so that we cannot *à priori* assign a value to μ. But a probable estimate may be arrived at from the consideration that a great circle of the sphere cuts, on an average, at least three lines of weakness, as appears upon inspection of a map showing the lines of volcanic action.

Hence

$$BB' = 8000 \text{ miles,}$$

whence

$$\mu = 2\,\frac{P_X - P_A}{8000}\,k$$

$$= \frac{P_X - P_A}{10}.$$

In this case the work on a square mile of section, viz. $\dfrac{P_X{}^2 - P_A{}^2}{\mu} ke$, will be expressed by $10(P_X + P_A)ke$.

The superior limiting case will be when the compressing force has the maximum value P assigned to it, and when P_A has likewise the same value. In that case, however, $\mu = 0$, or there is no action between the crust and nucleus, and, because $P_A = P_X$, no relief given at the weak plane A. This is therefore an impossible case. If, however, we were to suppose it to occur, we should have the work

$$= 20Pke.$$

This would be only five times as great as the work was found to be in the superior limiting case with a solid earth, and consequently would not afford 500 units of heat to a square foot of vertical section.

Again, if we make the other supposition made before, that $P_X = 400$ and $P_A = 200$, we have $\mu = 20$; and the coefficient analogous to that of friction being $\dfrac{\mu}{k}$, would be $\frac{1}{20}$, which seems a not improbable value as between the solid crust and a viscous layer. The work on a square mile of section in this case would be

$$10 \times 600 \times ke,$$

which is $\frac{9}{20}$ of the work in the superior limiting case as found for a solid earth, and would afford only 45 units of heat per square foot of section. This last may be looked upon as a not improbable estimate of the circumstances affecting the development of heat along a vertical section of the earth's crust upon the supposition that it rests upon a viscous layer; and we see that it would be quite incompetent to produce volcanic phenomena; at the same time the limiting case shows that no conceivable larger value of the compressing force would raise the amount of heat sufficiently for that purpose; so that in this case, as well as in that of a solid earth, we conclude that the theory is untenable.

If the work of descent of the crust is not transformed into the heat of volcanic energy, it may be asked what becomes of it? The answer is, it is transformed

      (1) into heat within the nucleus,
      (2) into work of relative elevation in the crust,
      (3) into heat within the crust.

This last is the only portion which can be appealed to for volcanic energy. The amount of it is probably too small (being on the most extravagant supposition only $\frac{1}{15}$ of the whole) to

have any appreciable effect in producing the observed inequalities of subterranean temperature.

We appear, then, to be thrown back upon the old explanation of volcanic phenomena, which supposes them to be manifestations of the intense heat existing at great depths and conveyed to the surface through channels of communication. And it appears to me that the results obtained upon making a comparison between the actual inequalities of the earth's surface and those which would have been formed if the earth had cooled as a solid body, strengthen this supposition*. They have led me to the conclusion that the interior of the earth has been in some condition which has admitted of its shrinking much more than it would have done if it had cooled as a solid ; nay probably more than mere cooling alone can account for. I surmise, therefore, that the nucleus has contained water, which has escaped through volcanic vents as superheated steam, and that the earth's volume has been diminished in that manner. By the presence of such highly heated water I would account for volcanic action still. It is known that in several vents lava remains permanently fused without permanently overflowing. This shows that heat is brought up from below, not by continual supplies of melted rock, but by intensely heated gases passing through the lava.

Under this view any disruption in the crust which is sufficient to permit the passage of steam at an enormous pressure, would originate a volcano. And much of the lava poured out might consist of the materials of the crust itself, fused by the passage of the gases through it, and so vary in its composition at different vents, and even at the same vent at different times. Various kinds of disruption of the crust may be conceived which

* See a paper by the author " On the Inequalities of the Earth's Surface, viewed in connexion with the Secular Cooling," Cambridge Philosophical Transactions, vol. xii. part. 2.

would open the necessary communication, and especially cross fractures combined with two orthogonal shifts, as in the figure. This may easily be illustrated with four books, laid corner to corner and shifted as shown. Indeed a steam-tight envelope can only be maintained under great compression, and any disturbance would tend to allow a passage through it. Thus the difficulty of getting the heat up from below seems capable of being explained away ; and many of the phenomena of metamorphism may be accounted for by the percolation of steam and highly heated water, which, not having reached the surface at a sufficient temperature, may have failed to establish a volcanic vent.

XXXVII. *Notices respecting New Books.*

On the Sensations of Tones as a Physiological Basis for the Theory of Music. By HERMANN L. F. HELMHOLTZ, *M.D. Translated, with the Author's sanction, from the Third German Edition, with Additional Notes and an Additional Appendix, by* ALEXANDER J. ELLIS, *F.R.S.*

WE have now before us the translation of the third edition of Prof. Helmholtz's 'Tonempfindungen' by Mr. Ellis. Though the English scientific public has for some time been acquainted with the principles and methods of this work through Mr. Sedley Taylor's able exposition, it was a want much felt that we had no translation of the original Treatise.

This want has now been supplied in a clear and forcible translation, which, besides the advantage of easy and pleasant English, has the additional advantage of avoiding such words as timbre, and clang, and clangtint, which we agree with Mr. Ellis in considering unnecessary as our scientific nomenclature now stands.

The purpose of this book is to give, so far as this is possible, a physical explanation of music and of the physical materials at the command of the musician.

It sets forth how much our organs of hearing can do, what objective facts are presented to them, and how from the two we can explain the analysis of Sounds, Concord and Discord, Melody and Harmony.

The first section of the work discusses partial tones, with their influence on quality of tone, and also the physiology of the ear, with the object of discovering how we are able to analyze and combine sounds. The waves of the sea are seen moving across each other and coalescing, then separating again, and each pursuing its own path ; the whole process of interference is here apparent to the eye, but the ear cannot so survey the waves of sound. The passages of the ear are so small compared with the waves of sound, that only those motions in the directions of the axis are available for disturbing the air in it. But though the ear cannot survey the process of interference, it yet possesses the

power of separating and recognizing individual tones in a composite mass of tones, in such a way that Ohm was led to enunciate this law :—"Every motion of the air which corresponds to a composite mass of musical tones is capable of being analyzed into a sum of simple pendular vibrations corresponding to a simple tone sensible to the ear, and having its pitch determined by the periodic time of the corresponding motion of the ear." If we direct our attention closely to the tones of a piano or violin, we become conscious of higher tones mingling with the fundamental tone ; and the more we accustom ourselves to discriminating these, the easier and fuller is our recognition of them. In Prof. Helmholtz's theory their presence is considered as determining quality of tone ; and they are also, in this theory, the chief cause of the difference between consonant and dissonant intervals.

It is therefore an important inquiry, are there any objective signs of their existence ? and if so, how can we most easily and distinctly apprehend them ? The phenomena of sympathetic resonance are a full answer to the first question. They all depend on the power of a vibrating body to set in vibration even a large mass, provided that this mass have a proper period of its own the same as that of the vibrating body. Thus a large bell may be set ringing by a very small force, provided that the pulls are of the same period as the proper swing of the bell ; and a very small forward and backward motion of the hand will put into vibration a ball which it holds suspended by a thread, provided only that the proper period of the ball pendulum coincides with one of the periods of the hand's motion when resolved into simple pendular vibrations by Fourier's theorem.

It is the same with the small vibrations of sound. Take for example a piano-string sounding g' ; the string C being struck and then damped, g' is heard to sound. Here the vibration of the sounding-board gives small impulses to the string of g' at its points of attachment ; these impulses accumulate, each going to help the previously acquired swing of the string, and the result is the tone g'.

By means of his globe resonators, which strongly reinforce tones in unison with their proper note, Prof. Helmholtz was able to examine the upper partials of different notes. All these cases are instances of a mechanical selection of simple pendular vibrations.

The ear at first experiences great difficulty in detecting upper partials, because the whole training of ordinary ears is towards fusing them, and discriminating the fused results from each other. But we may train ourselves to detect them by such expedients as first sounding the higher harmonics, and then the prime tone of the harmonic, or by first detecting them with the resonator, and then fixing our attention on them when the fundamental tone is sounded. Having in this way trained ourselves to detect the harmonics, we may then do without assistance.

By comparing the upper partial tones of various instruments with their quality of tone, these general results are obtained.

Simple tones are soft and pleasant, but dull. Musical tones, accompanied by a moderately loud series of the low upper partials, are comparatively *rich*. If the even partials are wanting, the tone is *hollow* and *nasal*. When the prime tone is weak compared with the upper partials, the quality of tone is *poor*.

The vowel-sounds are tones produced by the vocal chords, with a resonance-chamber capable of altering in length, width, and resonant pitch, and therefore capable at different times of reinforcing different partials of the compound tone. By means of his resonators, Prof. Helmholtz has examined the upper partials of the different vowels, and has been able, by means of a combination of tuning-forks and resonant chambers, to construct some of the vowels, as U O Ö.

The last part of this division of the work is occupied with the Physiology of the Ear, which is very fully discussed, and seems to justify us in concluding that in the basilar membrane with Corti's arches, we have a stringed instrument capable of performing all the analysis accomplished by the finest ears.

The second division of the work concerns itself with the nature of combinational tones and beats, as well as the manner in which they distinguish Consonance and Dissonance.

Combinational tones arise when the aërial displacements are so large that they can no longer be taken proportional to the forces of restitution.

Suppose a particle of mass m to be impinged on by two sonorous waves, with forces $f \sin pt$ and $g \sin (qt+c)$ in the same direction, and that the force due to its displacement x is $ax+bx^2$, we have for its equation of motion

$$-m\frac{d^2x}{dt^2} = ax+bx^2+f \sin pt+g \sin (qt+c).$$

Integrating this equation by successive approximations, we find two vibrations of the respective periods $\overline{p-q}$ and $\overline{p+q}$. The former are the differential tones of Tartini; the latter, discovered by Prof. Helmholtz, he calls summational tones. The summational tones are much weaker than the differential tones; and both, by their combination with the prime tones, give rise to weaker combinational tones.

These tones are objective, and can be reinforced by resonators tuned to be in unison with them.

Beats, again, are subjective; they are produced by two tones nearly of the same pitch; and if Prof. Helmholtz's theory of the function of Corti's arches is correct, they are due to the same Corti's arches being set in vibration by the two tones. The number of beats per second is equal to the difference of the vibrational numbers of the interfering tones, this being the number of times that the tones reinforce each other. The disagreeable character of beats is to be ascribed to the fatigue which they cause in the ear by the intermittent excitement they produce. Dissonance, then, arises from the upper partials of two tones being so near in pitch

that beats arise; and consonant intervals are those in which no beats are formed, or, at least, such as produce no unpleasant disturbance.

Most musical tones being rich in upper partials, the consonant intervals are limited by the beats arising when one or both tones are imperfect; and in those instruments which are deficient in upper partials, such as tuning-forks and stopped organ-pipes, the required limitation is to some extent supplied by the combinational tones.

Again, the superiority of the major chord over the minor is mainly due to the combinational tones of the first order being audible in the latter.

Rameau and D'Alembert were the first to recognize the importance of partial tones in limiting the consonant intervals, about the middle of last century; and Tartini showed the importance of combinational tones; but their conclusions, though important advances, were imperfect and partial.

The last division of the book consists of an historical survey of the progress of music, mainly with a view to the application of the physical principles established in the first two parts. The tonal relationship which was the characteristic of European music, and developed into the dependence of all the chords of a piece on the fundamental chord, is to be regarded as a principle of style, in the same way that the Gothic style of architecture is due to æsthetical feelings, and not to physical principles. The structure of the different parts of the musical edifice, however, is governed by physical laws quite as much as is the construction of great buildings.

It is impossible to enter fully into this part of the work, or to to do more than merely allude to the leading discoveries of its distinguished author; so we shall content ourselves with saying that this treatise will henceforth be a necessary part of the study of any English student wishing to master the theory of music.

The mathematics of the work are conveniently placed at the end of the book, along with a valuable collection of notes by the translator, among which we may specially mention those on Prof. Helmholtz's vowel-theory.

Climate and Time in their Geological Relations: a Theory of Secular Changes of the Earth's Climate. By JAMES CROLL, *of H. M. Geological Survey of Scotland.* London: Daldy, Isbister, and Co. 1875.

Neither Mr. Croll nor his work requires any introduction to the readers of the Philosophical Magazine, and it is almost a superfluous task on our part to recommend the present book to favourable notice. For a long series of years Mr. Croll has been an honoured contributor to these pages; and a great proportion of the contents of " Climate and Time " will be found scattered over the volumes of the last ten or twelve years. Besides doing simple justice to himself, Mr. Croll has rendered great service to the advancement of science by the collection, systematic arrangement,

and publication of these papers. Since he (ten years ago) first propounded in these pages the central idea of his theory to account for the then mysterious phenomena of the glacial drift, he has gone on patiently and successfully, not only buttressing his position by additional facts, arguments, and illustrations, but also enlarging its scope and widening its sweep till it has attained the solidity and dimensions presented in the present work. The publication of this volume marks one of the great eras in the progress of geological investigation. It is only necessary to compare a manual of physical geology of fifteen years ago with what the same ought to contain at the present day, to see how enormous is the modification made in fundamental principles, what a number of crude tentative speculations have given place to solid well-compacted theory, and how many loose and helpless-looking facts have by such theory found their fitting place and explanation. And when it is asked to whom science is chiefly indebted for this extension of its borders, who has made the most powerful and lasting impress on modern geological speculation, the answer undoubtedly should be—Mr. James Croll. No greater clearing of ground, lengthening of cords, and strengthening of stakes in the fields of geology have taken place since the days of Hutton; and while the Scotch may honour their Lyell, Murchison, Ramsay, and Geikie, they have the highest reason to be proud of their Hutton and Croll.

The manner in which Mr. Croll meets and combats Dr. Carpenter's theory of oceanic circulation is very characteristic of the unwearying patience, acuteness, and courage of his investigations. It is not our purpose here to express any opinion on this unfinished controversy, further than to say that Mr. Croll follows Dr. Carpenter into every one of his positions with a resolution and tenacity of purpose which, were it not so really calm and passionless, might almost be looked on as cruel and unmerciful. When, according to his own view, he has completely overthrown any of Dr. Carpenter's arguments by one special line of attack, he opens fire upon it from another point with the same hearty goodwill he might be expected to exhibit in leading a gallant onslaught against any hitherto unassailed position. This he does not do with the intention of thrice slaying the slain, but simply because he has in an eminent degree the faculty of examining a problem from many sides; and he is concerned, not for dialectic triumphs, but for the presentation of truth in its entirety. No sooner has the active and versatile Doctor marshalled new arguments from new facts in favour of his vast conception, than Mr. Croll is ready to take up his challenge, and show that all the facts can be read off in a light that harmonizes with his adopted wind theory. Mr. Croll has not the literary dexterity and skill which eminently characterize Dr. Carpenter's pen; but he, as much as any man, knows the manifold physical bearings of the vexed question; and while on the one side the exposition of an argument is adorned with literary grace, on the other the real scientific value of facts and observations is more promptly apprehended.

Since Mr. Croll first propounded his theory of secular changes of climate, a great amount of attention has been given to the arrangement and nature of superficial geological deposits; many old facts have been read in the new light shed by additional observation; and gradually much that was obscure, involved, and perplexing has come to take its fit and proper place. These observations have not in any case been made with the view of fitting the facts into Mr. Croll's theory, but were in many cases simply put down by altogether unprejudiced minds as so many isolated facts, which might hereafter be pieced together. Thus by degrees there have accumulated a great variety of observations, all tending to prove that the glacial epoch was as peculiarly characterized by periods of abnormal heat as it was by cycles of rigorous cold. The inevitable inference was at first very staggering to geologists, and they were naturally slow to admit this last and crowning mystery into the long train of perplexities which beset the reading of these baffling records. The discovery of numerous proofs of these warm interglacial periods has been one of the best testimonies to the soundness of the theory of secular changes, as it is the only theory which not only permits, but requires the occurrence of such oscillations of temperature. Mr. Croll has collected a formidable body of facts proving the interposition of such warm periods; and he has ingeniously and successfully accounted for the rarity and obscurity of the evidences of interglacial heat as compared with glacial cold. With the light so obtained he casts his eye backward over the whole geological record, and finds probable evidences of numerous glacial periods in the earth's history, one of the most important and interesting of which forms the Carboniferous period. But this is an inquiry which is yet in its infancy, although it is not difficult to suppose that results of the highest value may yet arise out of its prosecution.

Had Mr. Croll written his book entirely *de novo*, it is more than probable he would have succeeded in arranging his matter and in marshalling his facts and arguments in a more systematic and effective form. As it is, his work resembles more a storehouse than a museum: matter of great weight and value is packed up in its chapters; but these chapters have been in some cases fitted together more with a view to convenience than to their natural sequence and interdependence. This want of continuity would detract greatly from the value of the book with popular or holiday readers; but in no sense does Mr. Croll address such an audience. His work is eminently one for study: it is not simply to be read, but dug into; and while it will always hold its place among the standard records of science, it will for many a day to come be an indispensable work of reference to all cultivators of the more recondite problems of physical geography and geology.

XXXVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 154.]

June 24, 1874.—John Evans, Esq., F.R.S., President,
in the Chair.

THE following communications were read:—

18. " The Glacial Phenomena of the Eden Valley and the Western Part of the Yorkshire-Dale District." By J. G. Goodchild, Esq.

This paper is a continuation, in a northward direction, of the investigation of glacial phenomena which formed the matter of a paper lately read before the Society by Mr. Tiddeman, and published in the Society's Journal. It gives a detailed description of the district treated of, the occurrence of ice-scratches, glacial erosion, and glacial drift. The author considers that the phenomena recorded by him could not possibly have been produced by floating ice, and therefore must have been caused by land-ice. In the drift he finds evidence of a flow of ice from the south side of the Scottish southern uplands ; and the ice which filled the Eden valley seems to have had two opposite directions, the local ice flowing from the high land of the south pressing outwards far to the north of the currents which flowed eastwards—the latter currents of ice going up the Eden valley, but apparently not having ground down the ridges of the high land at the head of the valley. At the same time the author proves that it must have passed over to the lower levels, and that it undoubtedly deepened many of the valleys which happened to lie in its course. He considers that the Scotch ice could not well have exceeded 2400 or 2500 feet in thickness, and finally arrives at the conclusion that angular moraine-like drift of the high ground, the upper and lower tills, the deposits which form the eskers, and the numerous stranded boulders are the result of the melting of a vast thickness of land-ice charged throughout with stones and boulders of nearly every kind of rock occurring within the area in which the body of the ice originated.

19. " Geological Observations made on a visit to the Chaderkul, Thian-Shan range." By Dr. F. Stoliczka, F.G.S.

In this paper the author gives an account of the geology of the district traversed by him in his journey from near Kashgar to Lake Chaderkul on the Russian frontier, a distance of about 112 miles, his route lying among the southern branches of the Thian-Shan range.

Three principal ridges were crossed. The first, or " Artush ridge," consisted of newer Tertiary deposits of bedded clay and sand, mostly of a yellowish-white colour. These " Artush beds " were traced by the author for a distance of 22 miles. The southern slopes of this range were covered with gravel from 10 to 15 feet thick, which passes into a conglomerate with a thickness of about 200 feet.

The second, or " Kokan range," is formed on the southern side of

old sedimentary rocks, whilst the northern is occupied by newer Tertiary deposits and basaltic rocks—the former consisting of shales and limestones, in which the author found some fossils inducing him to refer them to the Trias. These are succeeded by some dark-coloured shales, slates, and sandstones, dipping at a high angle to the north.

On the denuded edges of these the newer Tertiaries rest, consisting of sandstones interstratified with basaltic rocks. These latter increase in thickness till just beyond Kulja an old " somma " is reached, with perpendicular walls rising to a height of 1500 feet above the river. The cone of the volcano has disappeared by subsidence.

The third ridge, "Terek-tagh," consists of old sedimentary rocks, chiefly limestones.

20. " Note upon a recent Discovery of Tin-ore in Tasmania." By Charles Gould, Esq., B.A., F.G.S.

In this paper the author states that the ore has been found in large masses *in situ*, and that the containing rock is lithologically distinct from the ordinary ternary granite which forms the whole of the stanniferous country of Queensland. The discovery was made at Mount Bischoff, in the N.W. portion of Tasmania; but, as the district is covered by scrub, an exact estimate of the exposed area of stanniferous rock is as yet impossible. It does not appear to exceed a few hundred acres. Large quantities of stream-tin have been already obtained.

21. " Note on the occurrence of a Labyrinthodont in the Yoredale Rocks of Wensleydale." By L. C. Miall, Esq.

The author briefly describes a specimen, discovered by Mr. W. Horne, of Leyburn, in the Lower Carboniferous Rocks there, comprising casts of five bones. Two of these are probably tibiæ. Owing to their fragmentary nature the others cannot be accurately determined. The author considers that these bones belong to an animal of higher rank than any known fish, and thinks that the Lower Coal-measures of Glasgow (with *Loxomma*) may be of earlier date than the Yoredale Rocks.

22. " Geological Notes on the Route traversed by the Yarkund Embassy, from Shahidulla to Yarkund and Kashgar." By Dr. F. Stoliczka, F.G.S.

The author described the rocks observed by him along the course of the Karakash river and through the Sanju pass as chiefly metamorphic and very highly inclined, until near Yám sedimentary rocks rest unconformably on the schists. These are probably Palæozoic. Later rocks occur near the camp Kiwáz, some resembling the rocks of the Nahún group and underlain by deposits containing Carboniferous fossils. At Sanju coarse grey calcareous sandstones and chloritic marls of Cretaceous age occur. True Löss occurs in some of the valleys.

23. " The Hematite Deposits of Whitehaven and Furness." By J. D. Kendall, Esq., F.G.S.

The deposits of hematite occur in the Silurian and Carboniferous rocks, but chiefly in the latter; and nearly all those worked in the two districts are found in the Mountain-limestone. They occur at all levels in the limestone, and generally near faults; their dip is the same as that of the beds in which they lie. Their longest axis almost always corresponds with the magnetic meridian. Their internal nature varies at the two localities. The Whitehaven hematite is much more compact than that of Furness. In the latter place it contains fossils from the Carboniferous Limestone.

The author considers the hematites to have been deposited by water, coming probably from the Coal-measures, containing bicarbonate of iron. The author believes that they were probably deposited after the Millstone-grit but before the Permian.

24. " Notes on the Physical Characters and Mineralogy of Newfoundland." By John Milne, Esq., F.G.S.

In this paper the author described in considerable detail the physical characters of the island of Newfoundland with regard to its surface-configuration and coast-outline, the traces of ice-action displayed in it, and its mineral resources. The details of the latter are given under the heads of the various localities visited by the author.

25. " Notes on the Sinaitic Peninsula and North-western Arabia." By John Milne, Esq., F.G.S.

In this paper the author described in great detail the geological observations made by him during a journey in company with Dr. Beke in the Sinaitic peninsula.

26. " Giants' Kettles at Christiania." By MM. W. C. Brögger and H. H. Reusch.

The authors first refer to the popular notices about Giants' kettles, and describe in detail a number of these pits, which were examined and emptied near Christiania. They then mention the theories concerning their origin. From their own facts and reading they conclude that many of these remarkable pits were made at the bottom of " Moulins " during a glacial period, when the locality was covered with ice on the scale of existing ice in Greenland. The contents of these pits are traced to their parent rocks, which are higher up towards the great valley of Gudbrandsdal, in which glacial phenomena abound. They are inclined to conclude that morainematter was washed off the glacier-ice from time to time, and left in the pits at last.

November 4, 1874.—John Evans, Esq., F.R.S., President, in the Chair.

The following communication was read:—

" Notes on the Comparative Microscopic Rock-structure of some Ancient and Modern Volcanic Rocks." By J. Clifton Ward, Esq., F.G.S.

The author stated at the outset that his object was to compare

the microscopic rock-structure of several groups of volcanic rocks, and in so doing to gain light, if possible, upon the original structure of some of the oldest members of that series. The first part of the paper comprised an abstract of what had been previously done in this subject.

The second part gave details of the microscopic structure of some few modern lavas, such as the Solfatara trachyte, the Vesuvian lava-flows of 1631 and 1794, and a lava of the Alban Mount, near Rome. In the trachyte of the Solfatara, acicular crystals of felspar show a well-marked flow around the larger and first-formed crystals. In the Vesuvian and Albanian lavas, leucite seems, in part at any rate, to take the place of the felspar of other lavas; and the majority of the leucite crystals seem to be somewhat imperfectly formed, as is the case with the small felspar prisms of the Solfatara rock. The order of crystallization of the component minerals was shown to be the following—magnetite, felspar in large or small *distinct* crystals, augite, felspathic or leucitic solvent. Some of the first-formed crystals were broken and rendered imperfect before the viscid state of igneous fusion ceased. Even in such modern lava-flows as that of the Solfatara considerable changes had taken place by alteration and the replacement of one mineral by another, and this very generally in successive layers corresponding to the crystal outlines. The frequent circular arrangement of the glass- and stone-cavities near the circumference of the minute leucite crystals in the lava of 1631 was thought to point to the fact that after the other minerals had separated from the leucitic solvent, the latter began to crystallize at numerous adjacent points; and as these points approached one another solidification proceeded more rapidly, and these cavities were more generally imprisoned than at the earlier stages of crystallization. In the example of the lava of 1794, where the leucite crystals were further apart, this peculiar arrangement of cavities was almost unknown.

The third part of the paper dealt with the lavas and ashes of North Wales; and the author thought that the following points were established :—

1. Specimens of lava from the Arans, the Arenigs, and Snowdon and its neighbourhood, all have the same microscopic structure. 2. This structure presents a hazy or milky-looking base, with scattered particles of a light-green dichroic mineral (chlorite), and generally some porphyritically imbedded felspar crystals or fragments of such, both orthoclase and plagioclase. In polarized light, on crossing the Nicols, the base breaks up into an irregular-coloured breccia, the colours changing to their complementaries on rotating either of the prisms. 3. Finely bedded ash, when *highly altered*, is in some cases undistinguishable in microscopic structure from undoubted felstone. 4. Ash of a coarser nature, when highly altered, is also very frequently not to be distinguished from felstone, though now and then the outlines of some of the fragments will reveal its true nature. 5. The fragments which make up the coarser ash-rocks seem generally to consist of felstone, containing both orthoclase and pla-

gioclase crystals or fragments; but occasionally there occur pieces of a more crystalline nature, with minute acicular prisms and plagioclase felspar. 6. In many cases the only tests that can be applied to distinguish between highly altered ash-rock and a felstone are the presence of a bedded or fragmentary appearance on *weathered* surfaces, and the gradual passage into less altered and unmistakable ash.

In the fourth division of his paper the author described some of the lavas and ashes of Cumberland of Lower Silurian age.

With regard to these ancient lavas the following was given as a general definition:—The rock is generally of some shade of blue or dark-green, generally weathering white round the edges, but to a very slight depth. It frequently assumes a tabular structure, the tabulæ being often curved, and breaks with a sharp conchoidal and flinty fracture. Silica 59–61 per cent. Matrix generally crystalline, containing crystals of labradorite or oligoclase and orthoclase, porphyritically imbedded, round which the small crystalline needles seem frequently to have flowed; magnetite generally abundant, and augite tolerably so, though usually changed into a soft dark-green mineral; apatite and perhaps olivine as occasional constituents. *Occasionally* the crystalline base is partly obscured and a felsitic structure takes its place.

The Cumberland lavas were shown to resemble the Solfatara greystone in the frequent flow of the crystalline base, and the modern lavas generally in the order in which the various minerals crystallized out. In *external* structure they have, for the most part, much more of a felsitic than a basaltic appearance. In internal structure they have considerable analogies with the basalts. In chemical composition they are neither true basalts nor true felstones. In petrological structure they have much the general character of the modern Vesuvian lavas—the separate flows being usually of no great thickness, being slaggy, vesicular, or brecciated at top and bottom, and having often a considerable range, as if they had flowed in some cases for several miles from their point of eruption. Their general microscopic appearance is also very different from that of such old basalts as those of South Stafford and some of those of Carboniferous age in Scotland.

On the whole, while believing that in *some* cases the lavas in question were true basalts, the author was inclined to regard most of them as occupying an intermediate place between felsitic and doleritic lavas; and as the felstone-lavas were once probably trachytes, these old Cumbrian rocks might perhaps be called Felsidolerites, answering in position to the modern Trachy-dolerites.

A detailed examination of Cumbrian ash-rocks had convinced the author that in many cases most intense metamorphism had taken place, that the finer ashy material had been partially melted down, and a kind of streaky flow caused around the larger fragments. There was every stage of transition from an ash-rock in which a bedded or fragmentary structure was clearly visible, to an exceedingly close and flinty felstone-like rock, undistinguishable in hand specimens

from a true contemporaneous trap. Such altered rocks, however, were quite distinct in microscopic structure from the undoubted lava-flows of the same district, and often distinct also from the Welsh felstones, although *some* were almost identical microscopically with the highly altered ashes of Wales, and, together with them, resembled the felstone-lavas of the same country.

This metamorphism among the Cumbrian rocks increases in amount as the great granitic centres are approached ; and it was believed by the author that it took place mainly at the commencement of the Old Red period, when the rocks in question must have been buried many thousands of feet deep beneath the Upper Silurian strata, and when probably the Eskdale granite was formed, perhaps partly by the extreme metamorphism of the volcanic series during upheaval and contortion. The author stated his belief that the Cumbrian volcanoes were mainly subaerial, since some 12,000 feet of ash- and lava-beds had been accumulated without any admixture of ordinary sedimentary material, except quite at the base, containing scarcely any *conglomeratic* beds, and destitute of fossils. He believed also that *one* of the chief volcanic centres of the district had been the present site of Kenwick, the low craggy hill called Castle Head representing the denuded stump or plug of an old volcano.

The author believed that one other truth of no slight importance might be gathered from these investigations, viz. that neither the careful inspection of hand specimens, nor the microscopic examination of thin slices, would in *all* cases enable truthful results to be arrived at, in discriminating between trap and altered ash-rocks ; but these methods and that of chemical analysis must be accompanied by oftentimes a laborious and detailed survey of the rocks in the open country, the various beds being traced out one by one, and their weathered surfaces particularly noticed.

November 18, 1874.—John Evans, Esq., F.R.S., President, in the Chair.

The following communications were read :—

1. " On fossil Evidences of a Sirenian Mammal (*Eotherium ægyptiacum*, Ow.) from the Nummulitic Eocene of the Mokattam Cliffs, near Cairo." By Prof. Owen, F.R.S., F.G.S., &c.

The specimens described in this paper were obtained by Dr. Grant, of Cairo, in a block of the white limestone of the Cerithian Nummulitic zone, quarried extensively for building-purposes in the Mokattam Cliffs. They consisted of a few fragments of the base of the cranium and a cast of the entire brain with the commencement of the myelon. The author discussed the characters presented by these remains, which he regarded as having belonged to an extinct Sirenian, probably allied to *Halitherium*, which he proposed to name *Eotherium ægyptiacum*. The characters of the brain, as deducible from the cast, were detailed, and shown to be sirenian. By comparison with the brains of other Sirenia, the author was led to trace a progress in the cerebral characters of the animals of this type, from its

first known appearance in the Nummulitic formation of Egypt to the present day. He also inferred, from its presence in the Nummulitic limestone, that this rock had been deposited not far from a shore.

2. " On the Geology of North-west Lincolnshire." By the Rev. J. E. Cross, M.A., F.G.S.

The district treated of is that lying between the three rivers Humber, Trent, and Ancholme. The Liassic and Oolitic beds were described, from the Keuper (found in the bed of the Trent) to the Cornbrash (the highest Oolitic stratum existing on this line). The existence of the Rhætic beds was held to be doubtful; the bone-bed and the shell *Avicula contorta* have not been found. On the other hand, the Lower Lias has a large development; and the recently discovered Ironstones of Frodingham and Sennthorpe were shown to lie in this formation, the zone being that of *Amm. semicostatus.* Higher up the series the zone of *Amm. margaritatus* seems to be wholly wanting, and the Marlstone series has dwindled to a bed of 8 feet thickness, locally termed the *Rhynchonella*-bed. The Upper Lias is represented by clays not much explored.

As regards the Oolites, the " Lincolnshire Oolite" is the prevailing rock; but a lower band, called " Santon Oolite," was distinguished from it, containing a different fauna. Above the Lincolnshire Oolite a greenish clay, capped by Cornbrash, represents the great Oolite formation; and beyond this the alluvium of the Ancholme valley covers every thing, till the Chalk rubble and the Chalk wold rise above it to the eastward.

XXXIX. *Intelligence and Miscellaneous Articles.*

ON THE COLD BANDS OF DARK SPECTRA.
BY MM. P. DESAINS AND AYMONET.

WHEN a thin pencil of rays from a Drummond lamp is dispersed by a rock-salt prism, and the distribution of the heat in the spectrum thus obtained is studied, cold bands like those of the solar spectrum are not detected; and yet those bands can be developed. For this purpose it is sufficient if the rays, previous to their incidence on the prism, be compelled to pass through suitable absorbents. This statement was proved by one of us several years since. Water and saline solutions were the absorbents most frequently made use of.

We have resumed this investigation, and ask permission to submit to the Academy a few of the results we have obtained.

The source of heat in all our experiments has been the lamp of MM. Bourbouze and Wiesnegg; its use is more convenient and trustworthy than that of the Drummond lamp.

In a first series of experiments we studied the development of the lines in a spectrum formed by means of a rock-salt prism of 60°,

the rays having passed through a centimetre of water. The lenses of the apparatus were also of rock-salt.

In these circumstances we have seen manifested distinctly, in the dark portion of the spectrum, four cold bands, the distances from which to the extreme red were

$$19'{\cdot}8, \quad 30'{\cdot}6, \quad 42', \quad 52'.$$

These numerical indications have not, and cannot have, a degree of precision equal to that obtained in determining the position of the dark lines of the bright spectrum; but, such as they are, they make known very nearly the spot where the pile must be placed in order to find the band on which we wish to operate.

Most frequently, in our experiments, the pile was 30 centims. from the axis of the prism; and the breadth of the admission-slit was half a millimetre: viewed from the axis of the prism, it subtended an angle of $5'{\cdot}7$; and consequently each cold band made its effect sensible within an angular space equal to its own angular breadth increased by $5'{\cdot}7$. But the minimum of thermoscopic effect was observed when the middle of the band nearly corresponded with the middle of the aperture of the pile; and we always took for the position of the line that of the pile which corresponded to the minimum studied. We will add that the illumination-slit had also, in general, a breadth of half a millimetre.

Subsequently to the researches of M. Lamanski on the cold lines of the dark spectrum of the sun, one of us had sought to determine the position of some of those lines; and, according to his measurements, four of them are situated at distances from the extreme red sensibly equal to

$$19'{\cdot}1, \quad 30', \quad 44', \quad 51'.$$

These positions are the same as those of the cold bands developed in the spectrum of Bourbouze and Wiesnegg's lamp by a layer of water of 1 centim. interposed in the path of the rays. The coincidence here shown seems to assign to the water of the atmosphere a great part in the production of the cold bands in the dark portion of the solar spectrum.

We afterwards made another series of experiments, with the aim of comparing the actions exerted upon dark spectra by different solutions, which consisted of a solvent nearly inactive with respect to the development of the lines and of a dissolved substance which, on the contrary, was capable of determining their formation. The active substance was iodine; the inactive solvents were chloride of carbon, chloroform, and sulphide of carbon. These three liquids dissolve iodine copiously; and all three solutions have the same appearance. Interposing them in the path of the rays, in layers of 1 centim. thickness, we obtained some results which we will put together in the form of a Table. In these fresh experiments the prism and lenses were of flint glass.

Position of the cold bands produced by iodine dissolved in chloride
of carbon, sulphide of carbon, or chloroform.

	Chloride.	Chloroform.	Sulphide.
Position of the lines {	1 28	1 30	° — ′
	1 34	—	1 35
	1 55	1 57	1 56

We do not pretend that these are the only lines these solutions
of iodine can produce ; indeed we hope to complete our Table in a
subsequent communication. But the numbers it contains show
already the conservation of the action of iodine in its three solutions.

We will add in conclusion, that in all our experiments we always
compelled ourselves to study the action of the vessel full of the sol-
vent alone upon the region of the spectrum where the iodine-solu-
tion determined the production of a line, so as to satisfy ourselves
that the action of the solvent and of the whole refracting system had
no action in the production of the phenomenon, or at least very little
in proportion to that of the active substance properly so called.—
Comptes Rendus de l'Académie des Sciences, September 6, 1875, vol.
lxxxi. pp. 423–425.

EXPERIMENTS ON THE PLASTICITY OF ICE.
BY PROF. DR. FR. PFAFF.

The phenomenon of glacier-motion has induced most of the
natural philosophers who occupied themselves with it to institute
experiments on the behaviour of snow and ice in relation to pressure.
The brothers Schlagintweit and Tyndall were the first who made
experiments of the kind with respect to the behaviour of glaciers.
Subsequently Helmholtz described a series of beautiful experiments,
from which it resulted that, by strong pressure, snow could be con-
verted into ice, ice broken small could be converted again into a
homogeneous cylinder, and such a cylinder be pressed through
apertures of smaller diameter, and more of the like kind. It was
thereby proved that ice under strong pressure can be brought into
any form desired, and consequently presents the same plasticity on
the small scale as is exhibited on the large scale by the mighty ice-
streams of glaciers, which also adapt themselves to the narrowings
and widenings of the valleys along which they flow. The pheno-
menon discovered by Faraday in 1850, which was afterwards so
frequently discussed under the name of regelation, gave the key for
the explanation of this. So far as I know, none of those named
above, nor any other physicist has sought to determine the pressure
under which ice changes its form ; all have worked with great
pressure, which is in fact necessary in order quickly to obtain
visible results. Only Moseley * instituted several series of experi-
ments for the purpose of ascertaining at what pressure or pull ice
is rent asunder, is crushed, and its plasticity becomes perceptible—

that is, at what pressure displacement of the ice-particles occurs. He found that, in order to rend a cylinder of ice, a weight of from 70 to 116 lbs., or from $5\frac{1}{2}$ to 9 atmospheres, per square inch of the cross section was necessary, according to the temperature; to *crush* it by pressure 101·8 lbs. on the square inch were requisite ; and to produce shearing 97·89–118 lbs., or a pressure of 7·5–9 atmospheres, must act upon the square inch.   I have this winter (1875) made a series of experiments in order to obtain somewhat nearer numerical values on the requisite degrees of pressure which are capable of altering still more demonstrably the form of ice, since for glacier-motion the important point is just this—what is the *minimum* of pressure under which ice is still plastic or yielding.

The results which I have obtained are, to any one who keeps in view the brittle nature of ice, quite as striking as the mobility of such a mass of ice as a glacier appeared at first to every one striking and wonderful. They show, namely, that even the *slightest* pressure is sufficient to displace the particles, if it *acts continuously and the temperature of the ice and around it is near the melting-point.*   Even Moseley recognized the influence of temperature on the mechanical properties of ice, but clearly not to the full extent.

It follows from my experiments that ice, near its melting-point, behaves *indeed like wax,* and with a pressure of only two atmospheres shows itself so yielding, that *e. g.* a hollow cylinder of iron of 11·5 millims. diameter and 1·7 thickness of the sides penetrated in two hours, at a temperature between $-1°$ and $0°·5$, to a depth of 3 millims. into the ice.   Respecting the influence of the temperature I will cite only a few data from my experiments.

The same cylinder, under the same pressure, with a temperature between $-4°$ and $-1°$, sank in 12 hours to a depth of $1\frac{1}{4}$ millim.; while with the temperature varying between $-6°$ and $-12°$ the sinking in five days, under a pressure of 5 atmospheres, amounted to only 1 millim., consequently only $\frac{1}{10}$ millim. in 12 hours !

If the surrounding temperature rises above the melting-point, the softness of the ice becomes so great that in one hour the same iron cylinder, under an equally slight pressure, sank to a depth of 3 centims., although it was completely enwrapped in snow, in order to avoid the raising of the temperature of the cylinder above zero. That in fact melting of the ice (which of course, on account of the lowering of the freezing-point below 0° by pressure, cannot be entirely avoided) had scarcely a perceptible influence in this experiment was shown by this—that the inner cylinder of ice filled out the iron cylinder quite tightly, so that it would not fall out, but required to be pressed out, and scarcely a trace of liquid water appeared in the depression left when the iron cylinder was lifted.

The pressure was applied, in all the experiments, by means of a one-armed lever, consisting of a steel bar of rectangular cross section and 86 centims. length, which was perforated at its hinder end and fixed on a steel pin upon which it turned easily.   By this simple arrangement any pressure could be maintained constant as long as desired.

In another series of experiments, the temperature of the air being 2°·5 C., various hollow iron cylinders, and a solid piece of steel whose base had a surface of exactly 1 square centim., were placed upon ice, and this was then covered with snow to the height of about 1 foot. With a pressure equal to $\frac{1}{3}$ of an atmosphere upon a steel piece of 6·4 centims. superficies it sank in 3 hours 14 millims. into the ice, while the melting away of the surface during that time was barely measurable, amounting to $\frac{1}{4}$ millim. The quadrangular solid prism of steel, exerting a pressure of $\frac{1}{3}$ of an atmosphere, sank 4 millims. in 5 hours. It appears very clearly from all these experiments how considerable becomes the yielding of the ice itself to the slightest pressure with a temperature near its melting-point. We may hence infer that, at this temperature, the plasticity of ice sinks to zero only when the pressure likewise becomes *nil*, but with falling temperature falls very rapidly. To ascertain, however, in what ratio this ensues, a greater number of precise experiments than I have made would be needed.

It is still constantly assumed, on the ground of some of Tyndall's experiments, that ice is destitute of *extensibility* and flexibility, although repeated observations recently made compel us to ascribe to ice some flexibility. The oldest observation of this kind known to me originated with Kane, who remarked that a large lump of ice with its edges resting on two others became curved in the course of some months.

I made several experiments in a similar manner to that in which nature had operated previously to Kane. A parallelepiped of ice 52 centims. long, 2·5 wide, and 1·3 thick was placed with its two ends to the extent of 5 millims. resting on blocks of wood. From the 8th to the 15th of February, the temperature varying between —12° and 3°·5, the centre sank extremely little, yet decidedly perceptibly, from day to day, on the average from 2 to 3 millims. in 24 hours; so that the total sinking amounted on the 15th to 11·5 millims. From that time the temperature rose, but remained below zero till noon of the 16th; yet even this rise of temperature effected a quick increase of the flexure, as in 24 hours (from 8 A.M. on the 15th to the same hour on the 16th) the latter reached the amount of 9 millims. (consequently 20·5 in all). No rending of the ice was anywhere perceptible; even the under surface did not, on the most careful examination, show the minutest trace of a crack. Up to 2 o'clock P.M. the sinking increased 3 millims. The next inspection, at 5, during which time the temperature had risen to $+3°$, showed the bar broken into two pieces.

Consequently, at the last observation, the bending in a perpendicular direction (that is, the height of the perpendicular erected upon the chord of the arc at last formed by the ice) amounted to 23·5 millims., which certainly, considering that the length of the chord was but 51 centims., must be called very considerable, and at all events proves that ice is not so unyielding towards stretching and pull, provided this acts with sufficient slowness.

I next endeavoured to determine the amount of extension of ice by *traction*. To a prism of ice of the same dimensions as that em-

ployed for the bending, a weight of 3 kilogrammes was suspended beneath, and left to itself for 7 days (from the 11th to the 17th of February).   It was suspended in this way : at a distance of 3 centims. from each end an aperture was pierced with a hot wire ; and through this a string was drawn.   On the 16th, when the temperature rose, the resulting softness of the ice had this effect, that the string gradually cut through it.   Yet, between two marks quite close to the ends, consisting of two pointed pieces of wood frozen into the bar and with only the points protruding, up to that time a lengthening of the cylinder 1 millim. appeared.   By the string cutting through, the marks also changed their position, so that the further extension until the ice parted could not be with certainty determined.

It is therefore shown here also that a pull continued for a longer time, even when it is slight, stretches ice, that near its melting-point it shows itself like other bodies yielding to pressure as well as to pull, and especially that, towards the former, at a temperature in the vicinity of zero, it is to be regarded as an eminently plastic substance.

The phenomena of the motion of glaciers will after this appear less surprising ; and in like manner the behaviour of ice at different temperatures towards pressure throws a new light on the fact that the velocity of glacier-motion rises with the temperature.   Since the glacier-ice and the air over it possess, at least in the summer months, a temperature which departs very little from the freezing-point, a very slight pressure suffices to set it in motion ; and I think that the so-called sliding theory receives fresh support from the experiments above described.—Poggendorff's *Annalen*, vol. clv. pp. 169–174.

ON MUSICAL CONSONANCE.

To *William Francis, Esq., Ph.D.*

DEAR FRANCIS,

Would you kindly permit me, in your next Number, to express my indebtedness to Mr. Sedley Taylor, Professor Mayer of Hoboken, and Mr. Bosanquet for pointing out an error in the statement of Helmholtz's theory of Consonance in my eighth Lecture upon Sound?

It would be easy, if it were of any use, to show the origin of this mistake.   Suffice it to say that it has been long known to me, that it has been corrected in the last edition of my work on Sound, and that the corrected statement of the theory, though necessarily brief, is, I have reason to know, regarded by Helmholtz as "perfectly clear and exact."

With regard to the experimental data referred to in my eighth Lecture, I may have something to say on a future occasion.

I am, dear Francis,

Faithfully yours,

JOHN TYNDALL.

Royal Institution,
September 15, 1875.

XL. *A new Relation between Electricity and Light: Dielectrified Media Birefringent. By* JOHN KERR, *LL.D., Mathematical Lecturer of the Free-Church Training College, Glasgow*[*].

THE thought which led me to the following inquiry was briefly this:—that if a transparent and optically isotropic insulator were subjected properly to intense electrostatic force, it should act no longer as an isotropic body upon light sent through it. Faraday was often occupied with expectations of this kind; and he has mentioned in his memoir on the Magnetization of Light, and elsewhere in his 'Researches,' how he experimented in this very direction, upon electrolytes as well as dielectrics, at different times and in many ways, but always without success[†]. As far as I remember, I have not read or heard of an attempt in this field by any other naturalist. I proceed to offer a few notes of some recent experiments of my own. The investigation is not so complete as I should wish it to be; but it has been carried forward as far as my limited time and means would allow. At present I confine myself to solid dielectrics, reserving the case of liquids for a second paper. The principal results given in this first paper are stated apart, in articles 11, 17, 23.

1. *Dielectric of Plate Glass.*—A piece of good plate glass, $\frac{3}{4}$ inch thick, is formed roughly, before it leaves the shop, into a rectangular block 6 inches long and 2 wide. In this and subsequent operations, the original polish of the plate is carefully

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[*] Communicated by the Author.

[†] Faraday's 'Experimental Researches,' 2216, p. 951, or Maxwell's 'Treatise on Electricity,' vol. ii. p. 399.

preserved. Two holes, about $\frac{1}{16}$ inch wide, are drilled into the block from its opposite ends; they lie exactly as in continuation of each other, in a line parallel to the longest edges of the block, and midway between opposite faces; and they terminate in well-rounded bottoms at the centre of the block, with a short extent (a quarter inch or less) of clear glass between them. Two fine pillars of glass rise from a stand on the table, distant an inch or two from each other. The block is placed across the pillars (at about a foot from the table), its plate-faces vertical, and the line of borings horizontal; and in this position it is tied securely to the pillars by coils of silk thread. Two thick wires of copper, sheathed in gutta percha, have their bared extremities inserted in the borings, down to the ends. As a provision against the strongest electric action applied in any case, these wires are coated very deeply with lac or sealing-wax at their junction with the glass, and an inch or more outwards. The whole surface of pillars and block is well varnished with lac—except a small space which is left clean upon each of the plate-faces, to allow distinct vision through the centre of the block.

When the dielectric has been thus prepared, its transparency is all that can be desired. Objects bright or faint are seen horizontally through the central parts of the plate (between and around the ends of the borings) quite as well as through a fine window.

2. The electricity is obtained from a Ruhmkorff's induction-apparatus, which gives a spark of 20 to 25 centimetres. The dielectric just described stands upon the table close to the inductorium. The outer ends of the wires from the dielectric are screwed into the knobs of the secondary coil. From the same knobs two wires are led to the other side of the instrument, and are connected with two insulated metallic balls, which act as spark-terminals. The distance of these balls, or the actual spark-length in air, is under the observer's control at every instant.

The ends of the secondary coil are separated thus at one place by so many inches of air, and at another place by a quarter inch of glass. When the primary circuit is closed in the usual way, through the oscillating rheotome of the instrument, the air between the spark-terminals is broken by a sensibly incessant discharge, while the glass between the induction-terminals is traversed by a strong electric force. By simple increase or diminution of the distance between the spark-terminals, the intensity of electric strain thus produced at the centre of the glass block may be raised or lowered at once, and in any degree, as the observer pleases. I may mention that on several occasions, when the instrument was working at full power and the spark-terminals were drawn $7\frac{1}{2}$ or 8 inches apart, strong discharges burst

across between the two induction-wires, at the rate of about one per second, without cessation of the principal discharge. The insulation was so good that the dielectric was not in any way damaged. The discharges took place through the air in dense white sparks, from end to end of the block, a distance of 7 or 8 inches. In these circumstances, the part of the glass block between the induction-terminals must have been subjected to a strain little short of the utmost it could bear. Electric forces of such intensity were hardly ever applied in the experiments, were not indeed required.

3. The polariscope consists of two Nicol's prisms. A flat paraffin flame, presented edgeways, is used as a source of light. Next to the lamp and close to it comes the first Nicol; then at a distance of 2 feet or more comes the dielectric of plate glass; then at a like distance comes the second Nicol. The pieces are so arranged that the observer, looking horizontally through the polariscope, and keeping the first Nicol at the centre of the field of vision, sees the flame through the centre of the dielectric, midway between the induction-terminals. The light crosses the dielectric at right angles to the plate-faces, and therefore at right angles to the lines of force.

4. *Neutralizing Plate.*—Every very thick plate of glass which I have yet worked with, exerts at most of its points a slight depolarizing action upon transmitted light. When such a plate is inserted between the two Nicols without compensation, any thing like perfect extinction is generally unattainable, and the sensibility of the polariscope is lost.

The principal section of the first Nicol being in any desired position, and that of the second Nicol perpendicular to it, the dielectric (still unexcited though connected with the coil) is inserted properly between the Nicols, and the light reappears well in the polariscope.

The neutralizing plate, a piece of glass about 6 inches square, taken from the same original plate as the dielectric, is then placed upon a stand immediately in front of the second Nicol, and is moved by trial into such a position that the restored light is again extinguished perfectly by a very small rotation of the analyzer. The apparatus is now ready for work.

5. The highest powers applied in the experiments are sufficiently indicated thus:—battery in the primary circuit, a series of six Grove's or Bunsen's elements; corresponding spark-length 9 or 10 inches; actual spark-length, or thickness of air between the spark-terminals, 6 inches, rarely 7; thickness of glass between the induction-terminals $\frac{3}{16}$ inch. But powers a good deal lower give effects distinct enough.

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6. *First experiment.*—The pieces are arranged in the order and manner just described (2, 3, 4). The spark-terminals are fixed at a distance of 5 or 6 inches; the polarizing Nicol is laid with its principal section at 45° to the horizon; and the analyzer is turned, with the help of the neutralizing plate, into the position of perfect extinction. No piece of the apparatus is now touched, except the commutator, till the end of the experiment.

Looking through the polariscope, the observer closes the primary circuit. In about 2 seconds the light begins to reappear through the dielectric, at its old place between the induction-terminals, very faintly at first; but it brightens continuously for 10 seconds, 20, even 30, till it is almost brilliant. When the primary circuit is now broken by the commutator, the light fades away continuously, at first rapidly, then more slowly, to perfect extinction. The time that elapses (in the latter part of the experiment) between the opening of the primary circuit and perfect extinction in the polariscope depends very noticeably upon the intensity and duration of the electric action, increasing as these increase.

7. The light thus restored by electric action cannot be extinguished again, at any stage of the experiment, by any rotation of the analyzer either way.

8. *Second experiment.*—The polarizing Nicol is laid with its principal section either horizontal or vertical; the analyzer is turned into the corresponding position of perfect extinction; all the other arrangements and the procedure are as in the first experiment. There is now no regular effect obtained in the polariscope. In many cases, indeed, even when the strongest electric action (5) has been kept up for 20 or 30 seconds, any recovery of the light is very doubtful, rather a guess than a perception.

Small effects do sometimes present themselves; but they are trifling and irregular in comparison with those obtained in the first experiment. They are probably due to known causes, such as imperfection of adjustments, irregularities of molecular structure in the dielectric, possibly also slight changes of temperature. If a small and irregular allowance be made for one or more such disturbing influences, there is now no effect in the polariscope.

9. *Third experiment.*—Distance of the spark-terminals small (say, 2 inches), the arrangements otherwise as in the first experiment. The electric action is kept up for a minute or more, till the intensity of the restored light is certainly constant. The spark-terminals are then separated all at once to a distance of 6 inches, and in a second or two there is an evident increase of effect in the polariscope. Simple arrangements will be described

soon, which exhibit this increase of effect as a reappearance from extinction.

10. *Fourth experiment.*—The same as the first, except that the primary current, instead of being constantly in one direction, is regularly reversed by the commutator at successive equal intervals of time (say, every second). The optical effect is as good as in the first experiment, if not better.

11. *Summary.*—When plate glass is intensely dielectrified, and traversed by polarized light in a direction perpendicular to the lines of force, it exerts a partially depolarizing action upon the light, giving an effect which is much more than merely sensible in a common polariscope. There is a good regular effect when the plane of polarization is at 45° to the lines of force, no regular effect when the plane of polarization is parallel or perpendicular to the lines of force. Electric force and optical effect increase together. The optical effect of a constant electric action takes a certain time (apparently about 30 seconds in my observations) to reach its full intensity, which it does by continuous increase from zero ; and it falls again slowly to zero after the electric force has vanished. There is as good an effect with a rapid succession of contrary (Ruhmkorffian) electrizations as with a continued (Ruhmkorffian) electrization in one direction.

12. *Optical Compensator.*—Not having a regular instrument of this kind, I supply its place by a simple slip of glass held in the hands and subjected to varying stress. The action of strained glass upon transmitted light has been exactly determined by experiment. Compressed glass acts as a negative uniaxal crystal with its axis parallel to the line of compression ; stretched glass acts as a positive uniaxal with its axis along the line of tension.

Illustrative optical experiment.—All the pieces placed as in the first experiment (2, 3, 4), the plane of polarization at 45° to the horizon, the extinction in the polariscope perfect, and the dielectric always unexcited. Two additional pieces are introduced into the course of the beam—say, between the dielectric and the neutralizing plate. The first is a small square of thin plate glass, held edgeways in a vice with its surfaces perpendicular to the beam, and feebly compressed in the direction of its length, which is horizontal. When this piece is inserted, the light is well restored from extinction. The second piece is the compensator—a rectangular slip of plate glass, shaped like a common microscopic slide, but generally larger. It is held by the two hands in front of the neutralizing plate, with its surfaces perpendicular to the beam, and its long edges horizontal ; it is gently bent by the hands, the axes of the couples applied being perpendicular to the plate-faces, so that (say) the upper parts of the slip are extended horizontally, and the lower parts compressed ; and it is lowered

or raised so that the light, after traversing the first piece, is trans-
mitted to the analyzer through the upper or lower parts of the
second piece. Through the extended parts the light is weak-
ened, and, with a right degree of tension, extinguished perfectly,
and with a greater tension restored again; through the com-
pressed parts it is always strengthened.

Generally, when the directions of stress in the two pieces
are parallel, compression and tension counteract each other;
two compressions or two tensions reinforce each other. When
the lines of stress are at right angles, two compressions or
two tensions counteract each other; compression and tension
strengthen each other.

The action of a strained piece in the polariscope is most
distinct when the direction of the stress lies, as it does here,
midway between the principal sections of the two crossed Nicols.
There is no action when the direction of stress lies in either of
these planes.

There are several variations of the illustrative experiment that
ought to be noticed here for their bearing on what follows.
The first piece may be stationed on the other side of the di-
electric; it may even be removed altogether, and the com-
pression or tension applied to the dielectric plate itself in a hori-
zontal direction perpendicular to the beam; the results are then
the same as formerly. It appears thus, without reference to
theory, that horizontal compression of the first piece has always
to be compensated in the same way (that is, by horizontal ten-
sion of the second piece), whether the first action is applied
before the dielectric, or behind it, or in it.

The illustrative experiment takes its simplest form when di-
electric and neutralizing plate are both removed. It appears
thus that horizontal compression of a first piece has to be
compensated in the same way (that is, by horizontal tension of
a second piece), whether the other two mutually balanced pieces,
the unexcited dielectric and the neutralizing plate, are present
or absent. Accordingly, and without reference to theory, in
proceeding to characterize the birefringent action of the excited
dielectric by means of the compensating slip, I assume that the
mutually balanced actions of unexcited dielectric and neutralizing
plate are without effect, and therefore to be left out of account.

13. The compensating slips used in the following experi-
ments were of different sizes. But there was one which I
came at last to employ almost exclusively, as I found it well
adapted to the whole range of effects examined. It was a rect-
angular piece of very good plate, $\frac{5}{16}$ inch thick, 2 inches wide,
and 10 long. It had no sensible action in the polariscope while
unstrained.

14. *Fifth experiment.*—A compensating slip is introduced between the dielectric and the analyzer; all the other arrangements and the procedure are as in the first experiment (6). The plane of polarization being at 45° to the horizon, and the initial extinction in the polariscope perfect, the electric force is applied, and the light is well restored. When the compensating slip is now raised into the course of the beam, and stretched horizontally, the light is weakened, and, with a proper degree of tension, extinguished perfectly. As the electric action is continued an increasing force has to be applied to the compensator to produce extinction; but the force is always of the same kind—tension exactly or nearly parallel to the lines of electric force, or compression in a direction exactly or nearly perpendicular.

After a few trials I got this phenomenon to present itself with perfect regularity. The compensator is chosen carefully (13), and inequalities of temperature avoided. A strong electric action is kept up for any convenient time, 5 or 6 seconds to 30 or 40, till the light is well restored. The slip is introduced as in the illustrative experiment (12), and carefully bent, so that the part of it crossed by the light is stretched horizontally by a continuously and slowly increasing force. In these circumstances, and constantly, the light fades away from a very considerable brightness to an extinction as pure as I have ever got in a polariscope. After this it is almost superfluous to say that, when the compensating slip is compressed in a direction parallel to the lines of force, the light restored by electric action is always distinctly strengthened.

15. *Sixth experiment.*—Distance of the spark-terminals small (say, 3 inches), the other arrangements as formerly. The electric action is kept up for a minute or more, till the light in the polariscope is certainly constant. The neutralizing plate is then moved about, and the analyzer turned (through a small angle in any case), till the light is again well extinguished. All the optical pieces being left untouched, the primary circuit is broken; the light reappears in a few seconds, and increases continuously up to a certain permanent intensity; but the compensator, to produce extinction at any stage of this increase, has to be compressed in a direction parallel to the lines of force. And this is evidently as it ought to be; for what the compensator has now to do is to reinforce the failing action of the dielectric, an action which has been proved equivalent optically to compression along the lines of force (14).

16. *Seventh experiment.*—Distance of the spark-terminals small, 1½ or 2 inches. As in the preceding experiment (15), a constant effect of electric action is obtained in the polariscope, and the light again extinguished by the neutralizing plate, which

is then left untouched, as well as the other optical pieces, till the end of the experiment. The electric action being still kept up, the distance of the spark-terminals is suddenly increased up to 5 or 6 inches; the light soon reappears and is allowed time to come out distinctly. When the primary circuit is now broken, the light fades away to extinction, and afterwards reappears; but before and after this passing extinction the light has contrary characters, as tested by the compensator, being extinguished in the former case by tension parallel to the lines of force, and in the latter case by compression.

17. From the last three experiments, or simply from the first of them, interpreted by the illustrative optical experiment (12), we infer that the dielectrization of plate glass is equivalent optically to a compression of the glass along the lines of electric force. Dielectrified glass acts upon transmitted light as a negative uniaxal with its axis parallel to the lines of force.

18. *Eighth experiment.*—All the preliminary arrangements are as in the fifth experiment (14). A strong electric action (spark of 6 inches) is kept up without ceasing for 20 minutes, and all the optical pieces are left untouched for an hour, the induction-terminals being connected with each other through the secondary coil from first to last. One thing very noticeable under such conditions is, the length of time which the light takes to fade away to extinction. In the present experiment there is a distinct, though faint, effect in the polariscope, even as long as 30 or 40 minutes after the external electric action has ceased. The effect has a constant character from first to last: the light restored by electric action is always extinguished perfectly by a right degree of tension of the compensator in a direction parallel to the lines of force. With the standard compensator (13), there is not a very great effort required for extinction at any time during the whole hour of observation.

19. *Ninth experiment.*—The analyzer is mounted in such a way that it may be moved in different directions at right angles to the beam, without rotation; the other arrangements are as in the fifth experiment. When a good effect has been obtained through the centre of the electric field, the second Nicol is moved about so as to receive the light through different parts of the dielectric.

Keeping first to the perpendicular bisector of the line joining the terminals, the intensity of the optical effect diminishes as the distance from the centre of the field increases: and this is particularly noticeable at first, while the effect is rising; but the action has always the same character, is always neutralized by horizontal tension of the compensator. As far as my means would allow, I have assured myself of the fact that, as soon as

there is a good virtual compression of the dielectric at the centre of the field, there is an effect of the same kind, a virtual compression in the same direction, beginning to manifest itself all along the equator, but more faintly as the distance from the centre of the field increases. Out of this equatorial line, and well back from the wires, the direction of apparent compression of the dielectric changes from point to point of the field, and is at some points vertical, perpendicular to the line joining the terminals. Only at points very close to the induction-wires does the compensator fail decisively to extinguish or greatly weaken the restored light.

20. In connexion with the preceding facts, I may mention that under certain conditions, in the fifth experiment, the compensator does not extinguish the restored light all at the same time, but produces a very dark broad band, which descends from the outer parts of the field towards the centre as the tension of the compensator increases. This happens, for instance, though not very regularly, when the electric force is very intense from the beginning and the optical effect is just beginning to show itself; it happens also when the lines of force are inclined to the horizon, so that the fine vertical band of flame seen through the centre of the dielectric crosses the equator of the field at about 45°. In these cases there appears to be an unusually rapid variation of birefringent action through the part examined of the electric field.

I have now done with the dielectric of plate glass. Half a dozen other solids were tried; and I will conclude with a short account of the only two of them which gave results worth mentioning. The great difficulty was to get a sufficiently strong superficial insulation, the masses being too small. In only one case, that of Iceland spar, was the dielectric perforated by discharge without giving a clear effect; but I think the crystal had received a predisposing flaw in the operation of boring.

21. *Dielectric of Resin.*—This is a piece very similar to the dielectric of plate glass in form, size, and adjuncts. A quantity of clear amber-resin is kept at a gentle fusion for some time till it is free from air-bubbles; it is then poured into a suitable mould and left to cool. Two thick stocking-wires of steel, previously fixed in the mould, along one line through its ends, remain imbedded as induction-terminals in the solid resin, leaving less than $\frac{1}{4}$ of an inch of clear dielectric between them at the centre of the block. The polarized light enters and leaves the central part of the block through small squares of thin plate glass, which are in optical contact with the resin and parallel to each other.

This dielectric, even the best specimen of it, is far inferior

to the dielectric of plate glass. It gives evidence of permanent and irregular strain in the neighbourhood of the terminals; it exerts a pretty strong photogyric action in the polariscope, separating the blue and red by a small angle; it is also imperfectly transparent for very faint light; but its chief defect is, that it allows a spark-discharge over its surface, a length of 7 inches, before the distance of the spark-terminals has much exceeded $2\frac{1}{2}$ inches. With all these deficiencies, the dielectric of resin gives a definite and regular effect; and the action is contrary to that of glass.

All the arrangements are essentially as in the fifth experiment (14). The block is tied to two fine insulating pillars of glass; the induction-wires of the dielectric are connected with the knobs of the secondary coil; and the light is seen through the centre of the block, midway between the terminals. A pretty good initial extinction is obtained in the polariscope between the blue and red; the light is then well restored by electric action, and the compensator is introduced between the dielectric and the second Nicol. By tension of the compensator in a direction parallel to the lines of force, the restored light is constantly and distinctly strengthened; and by compression parallel to the lines of force, it is regularly and greatly weakened, though never quite extinguished.

To test the value of these indications, I repeated the illustrative optical experiment (12) with the block of resin put in place of the large block of plate glass; and the results were satisfactory. I varied the experiment also by manipulating the block of resin itself as a compensator against the small square of compressed glass, the resin being simply pulled or pushed gently at both ends; and the results were equally distinct. The lines of stress being parallel, compression of glass and compression of the resin always reinforced each other, compression of glass and extension of the resin always counteracted each other, but never down to perfect extinction. It is true that these last results might be due more or less to the thin plates of glass which limit the resin. From all the observations, I infer that dielectrization of resin is optically equivalent to tension of the resin along the lines of force.

22. *Dielectric of Quartz.*—This is a plate perpendicular to the axis, made as for ordinary experiments in the polariscope, thickness 3 millims., length 20. Two fine holes are drilled into the plate as in the dielectric of plate glass, their bottoms flat, and with $\frac{1}{16}$ of an inch of clear crystal between them. Wires of copper are inserted in the borings, and are fixed along with the crystal to simple bearings made of glass rods; and the whole piece is coated very deeply with fused lac, a narrow window

being left at the centre of the plate. The action of the crystal in the polariscope is well neutralized by a contrary plate of equal thickness; the other arrangements are as in the fifth experiment. With my rough apparatus the adjustments are so troublesome that I have executed only one short series of observations, of which I append all the notes preserved.

Distance of spark-terminals $\frac{1}{2}$ inch: a faint but clear restoration of the light from almost perfect extinction; the compensator not working distinctly; the insulation perfect.

Distance 1 inch: the light clearly restored by electric action, and then well extinguished by tension of the compensator parallel to lines of force. The insulation is now failing, sparks passing occasionally over the surface of the lac near the crystal.

Distance $\frac{3}{4}$ inch: the insulation still defective; the light well restored by electric action and then well weakened by horizontal tension, and strengthened by compression. From what I have seen in some other experiments, I think that the effects in this case and the preceding may have been produced wholly or partially by slight changes of temperature, due to occasional spark-discharge over the surface of the crystal.

Distance again $\frac{1}{2}$ inch: the insulation again good. In this case it is noticed that, before the electric force is applied, while the body of the light is barely perceptible in the polariscope, there is a short length of it from the apex downwards (due exactly through the centre of the electric field) which is perfectly extinguished. By electric action this upper part of the light is restored very clearly, and is then as clearly extinguished by tension of the compensator parallel to the lines of force.

23. Upon the whole, though better experimental results are desirable, I consider it proved that dielectrified quartz (like glass) acts upon transmitted light as if compressed along the lines of force, while dielectrified resin (unlike glass) acts as if extended along the lines of force.

24. *Theory.*—Faraday's views as to the constitution and function of dielectrics apply here very aptly.

When the induction-terminals are charged, the particles of the dielectric throughout the field are electrically polarized, and tend accordingly to arrange themselves end to end, and to cohere in files along the lines of force, just as iron filings do in a magnetic field. As far as this tendency of the polarized particles towards a file arrangement along the lines of force takes effect, there is a new molecular structure induced in the dielectric.

If we neglect the influence of ordinary strains transmitted from point to point of the solid, and assume, as a good first approximation warranted by facts (19), that the change of mole-

cular arrangement at each point is determined solely or princi-
pally by electric force at the point, we cannot easily suppose the
new structure in a dielectric, originally isotropic, to be any thing
else than uniaxal, symmetrical at each point with reference to
the line of force through the point. And, even in the case of an
æolotropic body, we may assume, as a simple and sure approxi-
mation to the truth, that the effect of electric force is to super-
induce a uniaxal structure upon the primitive structure.

The uniaxal structure thus induced by dielectrization has been
experimentally detected and characterized by birefringent action
in three cases.   As a matter of fact, it appears to be negative in
glass and quartz, but positive in resin.

The electric force has probably a certain resistance to over-
come, something analogous to coercive force in the case of mag-
netism.   A sensible time is therefore required for the develop-
ment of the uniaxal structure by electric action, and for its dis-
appearance after the electric action has ceased.   Under an in-
tense and long-sustained electric force, the new structure of the
dielectric may assume the character of a very stiff and perhaps
permanent set, analogous to permanent or subpermanent mag-
netism (18).   We shall see afterwards, as might indeed be
expected, that there is nothing similar to this in the phenomena
presented by dielectrified liquids.

Contrary electrizations rapidly succeeding one another exert
contrary actions of electric polarization, but conspiring actions
of molecular arrangement: they are therefore as effective as a
continued electrization in one direction; and a Ruhmkorff's coil
is as effective as an electrical machine of equal strength.

I have made some experiments, and have had a good many
reflections, bearing on other explanations of the phenomena;
and I think it not unlikely that strains due to the mutual actions
of intensely charged shells of the dielectric, or strains due to
changes of temperature, may have something to do with the
facts.   But in the meantime I offer the preceding remarks as a
sketch of what appears to me to be the only probable theory.

Glasgow, September 22, 1875.

XLI. *Studies on Magnetic Distribution.   By* Henry A. Row-
land, *of the Johns Hopkins University, Baltimore, Md., U.S.A.*

[Continued from p. 277.]

V.

LET us now consider the case of that portion of the bar which
is covered by the helix.   First of all, when the helix is sym-
metrically placed on the rod, equations (5) and (6) will apply.   As

Q''_ϵ is the quantity which is usually taken to represent the distribution of magnetism, being nearly proportional to the "surface-density" of magnetism, I shall principally discuss it.

In the first place, then, this equation (5) shows that the distribution of magnetism in a very elongated electromagnet, and indeed in a steel magnet, does not change when pieces of soft iron bars of the same diameter as the magnet are placed against the poles, *provided that equal pieces are applied to both ends*; otherwise there is a change. This result would be modified by taking into account the variation of the permeability &c.

Let us first consider the case where the rod projects out of the end of the helix, as in Tables V., VI., and VII. By giving proper values to the constants, we obtain the results given in the last column of the Tables. The agreement with observation is in most cases very perfect. We also see the same variation of r that we before noticed in the rest of the curves, and we see that it is in just the direction theory would indicate from the change of μ.

In these Tables we come to a very important subject, and one to which I called attention some years back—namely, the *change in the distribution when the magnetizing force varies, and which is due to change of permeability*. The following Tables and figures show this extremely well, and are from very long rods with a helix a foot long at their centre, as in the last three Tables. The bar in both these Tables was ·19 inch in diameter and 5 feet long. The zero-point was at the centre of the bar and of the helix. The Tables give values of Q'_ϵ for the magnetizing forces which appear at the head of each column, and which are the tangents of the angles of deflection of the needles of a tangent-galvanometer. Table VIII. only gives the part covered by the helix. Both Tables are from the mean of both ends of the bar.

TABLE VIII.

x.	Strength of magnetizing current.			
	·108.	·194.	·378.	·600.
0	2·7	3·2	·7	·6
1			·9	·6
2	2·4	2·7	·9	·8
3	3·3	3·9	1·7	·8
4	4·0	6·0	4·0	3·2
5	5·7	8·7	9·3	14·7
6				

TABLE IX.

$x.$	C. ·257.	B. ·363.	A. 1·303.
0			1·1
1	2·5		1·3
2		3·1	
3	7·2	4·1	2·1
4		5·9	4·0
5	6·1	8·2	9·6
6	7·7	10·9	18·6
7	7·9	11·5	21·3
8	6·5	9·0	16·8
10	10·0	15·0	27·4
12	6·2	10·9	20·9
15	5·0	9·8	21·5
18	2·0	4·7	14·8
30	2·0	3·6	16·5

Fig. 3.
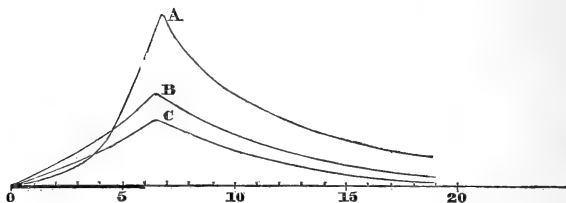
Plot of Table IX., showing surface-density for different values of the magnetizing force.

These experiments show in the most positive manner the effect we are considering ; and we are impressed by them with the great complication introduced into magnetic distribution by the variable character of magnetic permeability.

In fig. 3 I have represented the distribution on half the bar, as given in Table IX., the other half being of course similar. Here the greatest change is observed in the part covered by the helix, though there is also a great change in the other part. These Tables show that, as the magnetization of the bars increases, *at least beyond a certain point*, the curves on the part covered by the helix increase in steepness ; and the figure even shows that near the middle of the helix an *increase of magnetizing force may cause the surface-density to decrease* ; and Table VIII. shows this even better. Should we calculate Q'', however, we should always find it to increase with the magnetizing force in all cases. These effects can be shown also in the case where the bar does not extend beyond the helix, but not nearly so well as in this case, seeing that here Q'' can obtain a greater value.

Assuming that μ is variable, the formula indicates the same change that we observe; for as Q'' increases from zero upwards, μ will first increase and then decrease; so that as we increase the magnetizing force from zero upwards, the curve should first decrease in steepness and then increase indefinitely in steepness. In these Tables the decrease of steepness is not very apparent, because the magnetization is always too great; and indeed on this account it is difficult to show it; but in Tables V., VI., and VII. this action is shown to some extent by the values of r in the formulæ. The change of distribution with the helix arranged in this way at the centre of the bar is greater than in almost every other case, because the magnetism of the bar, Q'', can change greatly throughout the whole length of the helix, and thus the value of r be changed, and so the distribution become different.

The next case of distribution which I shall consider is that of a very long rod having a helix wound closely round it for some distance at one end.

Table X. is from a bar 9 feet long with a helix wound for one foot along one end. The bar was ·25 inch in diameter. All except the first column is the sum of two results with the current in opposite directions, and after letting the bar stand for some time, as indeed was done in nearly every case. The first column contains twice the quantities observed, so as to compare with the others. The zero-point was at the end of the bar covered by the helix.

Table X.

x and I.	A. ·245.	B. ·360.	C. ·600.	D. 1·09.
0	+17·6	+29·4	+52·0	+108·7
1	+ 9·6	+16·8	+31·5	+ 60·1
2	+ 7·4	+13·1	+24·3	+ 45·8
3	+ 5·4	+ 9·8	+19·1	+ 34·1
4	+ 3·4	+ 7·2	+14·7	+ 22·8
5	+ 2·0	+ 4·6	+ 9·9	+ 16·0
6	+ 0·6	+ 2·4	+ 5·4	+ 9·6
7	− 0·8	+ 0·3	+ 1·2	+ 0·6
8	− 1·8	− 1·6	− 2·1	− 0·3
9	− 3·0	− 3·6	− 6 6	− 8·8
10	− 5·0	− 6·3	− 8·6	− 15·6
11	− 7·4	−10·0	−16·4	− 27·1
12	− 8·4	−10·0	−16·9	− 26·5
13	− 6·0	− 7·9	−14·5	− 22·6
14	− 5·2	− 7·0	−12·5	− 21·0
15	......	− 5·3	−11·9	− 19·0
16	......	− 9·4	−19·1	− 31·2
18	......	− 5·3	−15·2	
20	......	− 6·5	−19·3	
24	......	− 5·6	− 6·0	
36	......	− 0·7	− 1·2	
48				

The value of Q''_ϵ between 0 and 1 includes the lines of force passing out at the end of the bar, and is therefore too large.

Fig. 4.
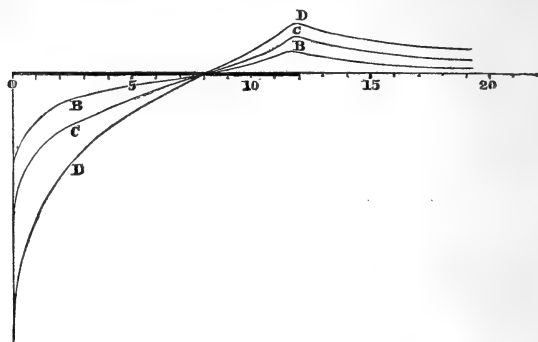
Plot of Table X.

In fig. 4 we have a plot of the results found for this bar. The curves are such as we should expect from our theory, except for the variations introduced by the causes which we have hitherto considered. Thus the sharp rise in the curve when near the end of the bar has already been explained in connexion with Table III. A small portion of it, however, is due to those lines of induction which pass out through the end section of the bar; and in future experiments these should be estimated and allowed for[*].

To estimate the shape of the curve theoretically in this case, let us take equation (4) once more, and in it make $s'=\infty$ and $s''=\sqrt{\mathrm{R R'}}$, which will make it apply to this case. We shall then have $\mathrm{A'}=-1$, and $\mathrm{A''}=\infty$, whence for the positive part of Q''_ϵ we have

$$Q''_\epsilon = -\frac{\mathfrak{H}\Delta\mathrm{L}}{2\mathrm{R'}r}\left\{-1+2\epsilon^{r(x-b)}-\epsilon^{2r(x-b)}\right\} = \frac{\mathfrak{H}\Delta\mathrm{L}}{2\mathrm{R'}r}\left\{1-\epsilon^{r(x-b)}\right\}^2,$$

and for the negative part

$$Q''_\epsilon = -\frac{\mathfrak{H}\Delta\mathrm{L}}{2\mathrm{R'}r}\left(1+\epsilon^{2r(x-b)}\right)\left(1-\epsilon^{-rx}\right);$$

therefore the real value is

$$Q''_\epsilon = -\frac{\mathfrak{H}\Delta\mathrm{L}}{2\mathrm{R'}r}\left(\epsilon^{r(x-b)}(\epsilon^{-rb}-2)+\epsilon^{-rx}\right).$$

---

[*] When considering surface-density, we should also allow for the direct action of the helix, though this is always found too small to be worth taking into account except in very accurate experiments.

And if x is reckoned from the end of the rod, we have

$$Q''_\epsilon = \frac{\mathfrak{H}\Delta L}{2R'r} \epsilon^{-r(b+x)} \{1 - 2\epsilon^{rb} + \epsilon^{2rx}\}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

When $x=0$, this becomes

$$\frac{\mathfrak{H}\Delta L}{2R'r} \epsilon^{-rb}(2 - 2\epsilon^{rb});$$

and when $x=b$, it becomes

$$\frac{\mathfrak{H}\Delta L}{2R'r} \epsilon^{-2rb}(1 - 2\epsilon^{rb} + \epsilon^{2rb}),$$

the ratio of which is

$$\tfrac{1}{2}(\epsilon^{-rb} - 1) ;$$

and this is the ratio of the values of Q''_ϵ at the ends of the helix. When b is 12 inches, as in this case, we get the following values of this ratio :—

$r=$	·05.	·1.	·15.	·20.	·30.	∞.
$-\tfrac{1}{2}(\epsilon^{-rb}-1)=$	·2256	·3494	·4173	·4546	·4863	·500
$\dfrac{-2}{\epsilon^{-rb}-1} =$	4·43	2·86	2·40	2·20	2·06	2·00

To compare this with our experiments, let us plot Table X. once more, rejecting, however, the end observations and completing the curve by the eye, thus getting rid of the error introduced at this point. We then find for this ratio, according to the different curves,

B.	C.	D.
2·1	2·3	3·2

It is seen that these are all above the limit 2, as they should be—though it is possible that it may fall below in some cases, owing to the variation of the permeability. As the magnetization increases, the values of the above ratio show that r decreases, as we should expect it to do from the variation of μ.

To find the neutral point in this case, we must have in formula (10)

$$\epsilon^{2rx} = 2\epsilon^{rb} - 1,$$

where x is the distance of the neutral point from the end. Making $b=12$, we have from this :—

$r =$	·05.	·10.	·15.	·20.	·30.	∞.
$x =$	10·1	8·96	8·31	7·89	7·39	6·00

By experiment we find that the neutral point is, in all the cases we have given in Table X., between 7·5 and 8·1 inches, which are quite near the points indicated by theory for the proper values of r, though we might expect curve D to pass through the point $x = 9$, except for the disturbing causes we have all along considered.

Our formulæ, then, express the general facts of the distribution in this case with considerable accuracy.

These experiments and calculations show the change in distribution in an electromagnet when we place a piece of iron against *one pole only*. In an ordinary straight electromagnet the neutral point is at the centre. When a paramagnetic substance is placed against or near one end, the neutral point moves toward it; but if the substance is diamagnetic it moves from it.

The same thing will happen, though in a less degree, in the case of a steel magnet; so that its neutral point depends on external conditions as well as on internal.

We now come to practically the most interesting case of distribution, namely that of a straight bar magnetized longitudinally either by a helix around it, or by placing it in a magnetic field parallel to the lines of force; we shall also see that this is the case of a steel magnet magnetized permanently. This case is the one considered by Biot (*Traité de Phys.* tome iii. p. 77) and Green (Mathematical Papers of the late George Green, p. 111, or Maxwell's 'Treatise,' art. 439), though they apply their formulæ more particularly to the case of steel magnets. Biot obtained his formula from the analogy of the magnet to a Zamboni pile or a tourmaline electrified by heat. Green obtained his for the case of a very long rod placed in a magnetic field parallel to the lines of force, and, in obtaining it, used a series of mathematical approximations whose physical meaning it is almost impossible to follow. Prof. Maxwell has criticised his method in the following terms ('Treatise,' art. 439) :— "Though some of the steps of this investigation are not rigorous, it is probable that the result represents roughly the actual magnetization in this most important case." From the theory which I have given in the first part of this paper we can deduce the physical meaning of Green's approximations; and these are included in the hypotheses there given, seeing that, when my

formula is applied to the special case considered by Green, it agrees with it where the permeability of the material is great. My formula, however, is far more general than Green's.

It is to Green that we owe the important remark that the distribution in a steel magnet may be nearly represented by the same formula that applies to electromagnets.

As Green uses what is known as the surface-density of magnetization, let us first see how this quantity compares with those I have used.

Suppose that a long thin steel wire is so magnetized in the direction of its length that when broken up the pieces will have the same magnetic moment. While the rod is together, if we calculate its effect on exterior bodies, we shall see that the ends are the only portions which seem to act. Hence we may mathematically consider the whole action of the rod to be due to the distribution of an *imaginary* magnetic fluid over the ends of the rod. As any case of magnetism can be represented by a proper combination of these rods, we see that all cases of this sort can be calculated on the supposition of there being two magnetic fluids distributed over the surfaces of the bodies, a unit quantity of which will repel another unit of like nature at a unit's distance with a unit of force. The surface-density at any point will then be the quantity of this fluid on a unit surface at the given point; and the linear density along a rod will be the quantity along a unit of length, supposing the density the same as at the given point.

Where we use induced currents to measure magnetism we measure the number of lines of force, or rather induction, cut by the wire, and the natural unit used is the number of lines of a unit field which will pass through a unit surface placed perpendicular to the lines of force. The unit pole produces a unit field at a unit's distance; hence the number of lines of force coming from the unit pole is 4π, and the linear density is

$$\lambda = \frac{Q_\epsilon}{4\pi\Delta L}, \quad . \quad . \quad . \quad . \quad . \quad (11)$$

and the surface-density

$$\delta = \frac{Q_\epsilon}{4\pi^2 d\Delta L}. \quad . \quad . \quad . \quad . \quad . \quad (12)$$

These really apply only to steel magnets; but as in the case of electromagnets the action of the helix is very small compared with that of the iron, especially when it is very long and the iron soft*, we can apply these to the cases we consider.

* I take this occasion to correct an error in Jenkin's 'Textbook of Electricity,' where it is stated that, by the introduction of the iron bar into

Transforming Green's formula into my notation, it gives

$$\lambda = \left(\frac{\pi d^2}{4}\right) \mathfrak{H}\kappa r \frac{\epsilon^{r(b-x)} - \epsilon^{rx}}{1 + \epsilon^{rb}}, \quad . \quad . \quad . \quad . \quad (13)$$

in which κ is Neumann's coefficient of magnetization by induction, and is equal to

$$\frac{\mu - 1}{4\pi}.$$

This equation then gives

$$Q''_\epsilon = \Delta L\left(\frac{\pi d^2}{4}\right) \mathfrak{H}r(\mu - 1)\frac{\epsilon^{rx} - \epsilon^{r(b-x)}}{1 + \epsilon^{rb}}. \quad . \quad . \quad (14)$$

Equation (5) can be approximately adapted to this case by making $s' = \infty$, which is equivalent to neglecting those lines of force which pass out of the end section of the bar. This gives $A' = -1$; hence

$$Q''_\epsilon = \Delta L \frac{\mathfrak{H}}{\sqrt{R R'}} \frac{\epsilon^{r(b-x)} - \epsilon^{rx}}{1 + \epsilon^{rb}}. \quad . \quad . \quad . \quad (15)$$

Now we have found (equation 7) that $r = \frac{2}{d}\sqrt{\frac{1}{\pi\mu R'}}$ nearly;
and this in Green's formula (equation 14) gives

$$Q''_\epsilon = \Delta L \frac{\mathfrak{H}}{\sqrt{R R'}} \frac{\mu - 1}{\mu} \frac{\epsilon^{rx} - \epsilon^{r(b-x)}}{1 + \epsilon^{rb}}, \quad . \quad . \quad (16)$$

which is identical with my own when μ is large, as it always is in the case of iron, nickel, or cobalt at ordinary temperatures.

When x is measured from the centre of the bar, my equation becomes

$$\lambda = \frac{\mathfrak{H}}{4\pi\sqrt{R R'}} \frac{\epsilon^{rx} - \epsilon^{-rx}}{\epsilon^{\frac{rb}{2}} + \epsilon^{-\frac{rb}{2}}}. \quad . \quad . \quad . \quad (17)$$

The constant part of Biot's formula is not the same as this; but for any given case it will give the same distribution.

Both Biot and Green have compared their formulæ with Coulomb's experiments, and found them to represent the distribution quite well. Hence it will not be necessary to consider the case of steel magnets very extensively, though I will give a few results for these further on.

At present let us take the case of electromagnets.

For observing the effect of the permeability, I took two wires 12·8 inches long and ·19 inch in diameter, one being of ordinary iron and the other of Stub's steel of the same temper as when

the helix, the number of lines of force is increased 32 times. The number should have been, from a quite small number for a short thick bar and hard iron to nearly 6000 for a long thin bar and softest iron.

purchased. These were wound uniformly from end to end with one layer of quite fine wire, making 600 turns in that distance.

In finding λ from Q''_ϵ, the latter was divided by $4\pi\Delta L$, except at the end, where the end-section was included with ΔL in the proper manner. x was measured from the end of the bar in inches.

<div align="center">Table XI.</div>

<div align="center">Iron Electromagnet.</div>

$x =$ distance from end.	Q_ϵ. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0	22·5	41·1	33·9	−7·2
$\frac{1}{2}$	12·6	25·1	26·9	+1·8
1	19·3	19·3	18·9	−0·4
2	12·0	12·0	11·7	− ·3
3	6·6	6·6	7·1	+ ·5
4	3·9	3·9	4·0	+ ·1
5	2·9	2·9	1·7	−1·2
6				
		$4\pi\lambda = 42$.		

The observations in Table XI. are the mean of four observations made on both ends of the bar and with the current in both directions.

The agreement with the formula in this Table is quite good; but we still observe the excess of observation over the formula at the end, as we have done all along. Here, for the first time, we see the error introduced by the method of experiment which I have before referred to (p. 266) in the *apparently* small value of $4\pi\lambda$ at $x = ·75$.

On trying the steel bar, I came across a curious fact, which, however, I have since found has been noticed by others. It is, that when an iron or steel bar has been magnetized for a long time in one direction and is then demagnetized, it is easier to magnetize it again in the same direction than in the opposite direction. The rod which I used in this experiment had been used as a permanent magnet for about a month, but was demagnetized before use. From this rod five cases of distribution were observed :—first, when the bar was used as an electromagnet with the magnetization in the same direction as the original magnetism; second, ditto with magnetization contrary to original magnetism; third, when used as a permanent magnet with magnetism the same as the original magnetism; fourth, ditto with magnetism opposite; and fifth, same as third, but curve taken after several days. The permanent magnetism was given by the current.

TABLE XII.
Stub's Steel.

| x. | Electromagnet. | | | | Permanent magnet. | | | | | |
| | Magnetism same as original. | | Magnetism opposite to original. | | Magnetism same as original. | | Magnetism opposite to original. | | Same as third, after three or four days. | |
	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.
0	23·3	42·5	15·9	29·0	⎱14·4	13·7	4·8	4·6	12·8	12·2
½	11·5	23·0	7·7	15·4	⎰					
1	8·2	16·4	5·9	11·8	⎱ 8·2	8·2	4·0	4·0	7·3	7·3
1½	6·1	12·2	4·3	8·6	⎰					
2	7·4	7·4	5·5	5·5	5·3	5·3	2·9	2·9	4·8	4·8
3	3·6	3·6	2·7	2·5	3·0	3·0	1·6	1·6	2·9	2·9
4	1·7	·8	1·0	·5	2·2	1·1	·9	·4	2·0	1·0
6										

The observations in Tables XI. and XII. can be compared together, the quantities being expressed in the same unknown arbitrary unit. It is to be noted that the bars in Tables XI. and XII. were subjected to the same magnetizing force.

First of all, from these Tables and figures (p. 359) we notice the change in distribution due to the quality of the substance; thus in fig. 5 we see that the curves for steel are much more steep than that of iron, and would thus give greater values to r in the formula—a result to be expected. We also observe in both figures the great change in distribution due to the direction of magnetization. In the case of the electromagnet this amounts to little more than a change in scale; but in the permanent magnet there is a real change of form in the curve. It seems probable that this change of form would be done away with by using a sufficient magnetizing power or magnetizing by application of permanent magnets; for it is probable that the fall in the curve E is due to the magnetizing force having been sufficient to change the polarity completely at the centre, but only partially at the ends.

On comparing the distribution on electromagnets with that on permanent magnets, we perceive that the curve is steeper toward the end in electromagnets than in permanent magnets. At first I thought it might be due to the direct action of the helix, but on trial found that the latter was almost inappreciable. I do not at present know the explanation of it.

As before mentioned, Coulomb has made many experiments on the distribution of magnetism on permanent magnets; and so I shall only consider this subject briefly. I have already given one or two results in Table XII.

Fig. 5.
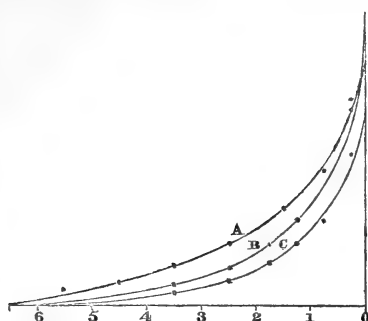
Results from electromagnets.
A. Iron, from Table XI.
B. Steel, from Table XII., magnetized same as originally.
C. Steel, from Table XII., magnetized opposite to its original magnetism.

Fig. 6.
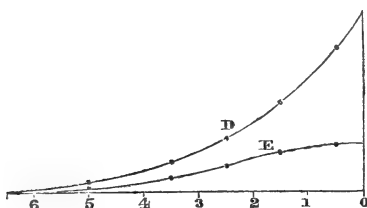
Results from steel permanent magnets.
D, magnetized in its original direction, Table XII.
E, magnetized opposite to its original direction, Table XII.
Scale four times that of fig. 5.

The following Tables were taken from two exactly similar Stub's steel rods not hardened, one of which was subsequently used in the experiments of Table XII. They were 12·8 inches long and ·19 inch in diameter.

TABLE XIII.

x.	Q_ϵ. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
1·28	46·6	34·9	34·26	−·6
2·56	23·8	18·6	18·60	0
3·84	12·6	9·8	9·88	+·1
5·12	7·2	5·6	4·77	−·8
6·40	2·3	1·8	1·41	−·4
$4\pi\lambda = {\cdot}117(10^{{\cdot}203(b-x)} - 10^{{\cdot}203x})$.				

Table XIV.

x.	Q_ϵ. Observed.	$4\pi\lambda$. Observed.	$4\pi\lambda$. Computed.	Error.
0				
1·28	42·6	31·9	30·74	−1·2
2·56	21·4	16·7	16·72	0
3·84	10·9	8·5	8·86	+ ·4
5·12	5·4	4·2	4·28	+ ·1
6·40	1·7	1·33	1·27	− ·1

$$4\pi\lambda = \cdot105(10^{\cdot203(b-x)} - 10^{\cdot203x}).$$

The coincidence of these observations with the formula is very remarkable; but still we see a little tendency in the end observation to rise above the value given by the formula.

In equation (7), and also from Green's formula, we have seen that for a given quality and temper of steel $p = \dfrac{rd}{2}$ is a constant. From Coulomb's experiments on a steel bar ·176 inch in diameter (whose quality and temper is unknown, though it was probably hardened) Green has calculated the value of this constant, and obtained ·05482, which was found from the French inch as the unit of length, but which is constant for all systems. From Tables XIII. and XIV. we find the value of r to be ·4674, whence $\dfrac{rd}{2} = \cdot04440$ for steel not hardened. As the steel becomes harder this quantity increases, and can probably reach about twice this for *very* hard steel.

To show the effect of hardening, I broke the bar used in Table XIV. at the centre, thus producing two bars 6·4 inches long. One of these halves was hardened till it could scarcely be scratched by a file; but the other half was left unaltered. The following Table gives the distribution, using the same unit as that of Tables XIII. and XIV. The bars were so short that the results can hardly be relied on; but they will at least suffice to show the change.

Table XV.

x.	Soft steel, A.		Hard steel, B.	
	Q_ϵ.	$4\pi\lambda$.	Q_ϵ.	$4\pi\lambda$.
0				
·64	20·4	29·1	47·7	68·1
1·28	9·8	15·3	13·9	21·7
1·92	6·0	9·4	7·0	11·0
3·20	3·8	3·0	2·6	2·0

Fig. 7.
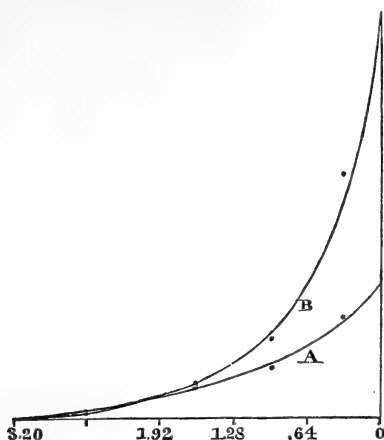
Results from permanent magnets.
A. Soft steel.
B. Hard steel.

In fig. 7 I have attempted to give the curve of distribution from Table XV., and have made the curves coincide with observation as nearly as possible, making a small allowance, however, for the errors introduced by the shortness of the bar. It is seen that the effect of hardening in a *bar of these dimensions* is to increase the quantity of magnetism, but especially that near the end. *Had the bar been very long, no increase in the total quantity of magnetism would have taken place ; but the distribution would have been changed.* From this we deduce the important fact *that hardening is most useful for short magnets. And it would seem that almost the only use in hardening magnets at all is to concentrate the magnetism and to reduce the weight.* Indeed I have made magnets from iron wire whose magnetization at the central section was just as intense as in a steel wire of the same size ; but to all appearance it was less strongly magnetized than the steel, because the magnetism was more diffused ; and as the magnetism was not distributed so nearly at the end as in the steel, its magnetic moment and time of vibration were less.

It is for these reasons that many makers of surveyors' compasses find it unnecessary to harden the needles, seeing these are long and thin.

We might deduce all these facts from the formulæ on the assumption that r is greater the harder the iron or steel.

Having now considered briefly the distribution on electro-

magnets and steel magnets, and found that the formulæ re-
present it in a general way, we may now use them for solving
a few questions that we desire to solve, though only in an
approximate manner.

VI.

M. Jamin, in his recent experiments on magnetic distribu-
tion, has obtained some very interesting results, although I
have [shown his method to be very defective. In his experi-
ments on iron bars magnetized at one end, he finds the formula
ϵ^{rl} to apply to long ones as I have done. Now it might be
argued that as the two methods *apparently* give the same re-
sult, they must be equally correct. But let us assume that
the attraction of his piece of soft iron F varied as some un-
known power n of the surface-density δ. Then we find

$$F = C\epsilon^{nrL},$$

which shows that the attractive force or any power of that force
can be represented by a logarithmic curve, though not by the
same one. Hence the error introduced by M. Jamin's method
is insidious and not easily detected, though it is none the
less hurtful and misleading, but rather the more so.

However, his results with respect to what he calls the nor-
mal magnet[*] are to some extent independent of these errors;
and we may now consider them.

Thus, in explaining the effect of placing hardened steel plates
on one another, he says, "Quand on superpose deux lames
aimantées pareilles, les courbes qui représentent les valeurs de F
[the attractive force on the piece of soft iron] s'élèvent, parce
que le magnétisme quitte les faces que l'on met en contact pour
se réfugier sur les parties extérieures. En même temps, les
deux courbes se rapprochent l'une de l'autre et du milieu de
l'aimant. Cet effet augmente avec une troisième lame et avec
une quatrième. Finalement les deux courbes se joignent au
milieu."

In applying the formula to this case of a compound magnet,
we have only to remark that when the bars lie closely toge-
ther they are theoretically the same as a solid magnet of the
same section, but are practically found to be stronger, because
thin bars can be tempered more uniformly hard than thick ones.
The addition of the bars to each other is similar, then, to an in-
crease in the area of the rod, and should produce nearly the
same effect on a rod of rectangular section as the increase of
diameter in a rod of circular section. Now the quantity $p = \dfrac{rd}{2}$

[*] "On the Theory of the Normal Magnets," *Comptes Rendus*, March 31,
1873; translated in Phil. Mag. June 1873.

is nearly constant in these rods for the same quality of steel, whence r decreases as d increases; and this in equation (17) shows that as the diameter is increased, the length being constant, the curves become less and less steep, until they finally become straight lines. This is exactly the meaning of M. Jamin's remark.

Where the ratio of the diameter to the length is small, the curves of distribution are *apparently* separated from each other, and are given by the equation

$$\lambda = \frac{\mathfrak{H}}{4\pi \sqrt{\mathrm{R}\mathrm{R}'}} \epsilon^{-rx}, \quad . \quad . \quad . \quad . \quad (18)$$

which is not dependent on the length of the rod. This is exactly the result found by Coulomb (Biot's *Physique,* vol. iii. pp. 74, 75). M. Jamin has also remarked this. He states that as he increases the number of plates the curves approach each other and finally unite; this he calls the "normal magnet;" and he supposes it to be the magnet of greatest power in proportion to its weight. "From this moment," says he, "the combination is at its maximum." The normal magnet, as thus defined, is very indefinite, as M. Jamin himself admits.

By our equations we can find the condition for a maximum, and can give the greatest values to the following, supposing the weight of the bar to be a fixed quantity in the first three.

1st. The magnetic moment.

2nd. The attractive force at the end.

3rd. The total number of lines of magnetic force passing from the bar.

4th. The magnetic moment, the length being constant and diameter variable.

Either of these may be regarded as a measure of the power of the bar, according to the view we take. The magnetic moment of a bar is easily found to be

$$\mathrm{M} = \frac{\mathfrak{H}}{4\pi r^2 \mathrm{R}'} \left\{ \frac{b}{2} - \frac{1}{r} \frac{1 - \epsilon^{-rb}}{1 + \epsilon^{-rb}} \right\}; \quad . \quad . \quad (19)$$

and if γ is the weight of a unit of volume of the steel and W is the weight of the magnet, we have finally

$$\mathrm{M} = \frac{\mathfrak{H}}{4\pi \mathrm{R}'\mathrm{C}^2} \left\{ \frac{1}{2} - \frac{1}{\mathrm{C}b^{\frac{3}{2}}} \frac{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} - 1}{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} + 1} \right\}, \quad . \quad . \quad (20)$$

where $\mathrm{C} = \dfrac{r}{\sqrt{b}} = p \sqrt{\dfrac{\pi\gamma}{\mathrm{W}}}.$

This only attains a maximum when $\dfrac{b}{d} = \infty$, or the rod is infinitely long compared with its diameter.

The second case is rather indefinite, seeing it will depend upon whether the body attracted is large or small. When it is small, we require to make the surface-density a maximum, the weight being constant. We find

$$\delta_0 = \frac{\mathfrak{H}}{\gamma \pi^2 \mathrm{R}' p} \; \frac{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} - 1}{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} + 1}, \quad . \quad . \quad . \quad (21)$$

which attains a maximum as before when $\dfrac{b}{d} = \infty$. When the attracted body is large, the attraction will depend more nearly upon the linear density,

$$\lambda_0 = \frac{\mathfrak{H}}{4\mathrm{C}\pi \mathrm{R}' \sqrt{b}} \; \frac{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} - 1}{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} + 1}, \quad . \quad . \quad . \quad (22)$$

which is a maximum when $\dfrac{b}{d} = \dfrac{1 \cdot 42}{p}$.

For the third case we have the value of Q'' at the centre of the bar from equation (6),

$$\mathrm{Q}'' = \frac{\mathfrak{H}}{2\mathrm{C}^2 \mathrm{R}' b} \; \frac{(\epsilon^{\frac{1}{2}\mathrm{C}b^{\frac{3}{2}}} - 1)^2}{\epsilon^{\mathrm{C}b^{\frac{3}{2}}} + 1}. \quad . \quad . \quad . \quad (23)$$

The condition for a maximum gives in this case

$$\frac{b}{d} = \frac{1 \cdot 65}{p}.$$

For the last case, in which the magnetic moment for a given length is to be made a maximum, we find

$$\frac{b}{d} = \frac{\cdot 1}{p}.$$

This last result is useful in preparing magnets for determining the intensity of the earth's magnetism, and shows that the magnets should be made short, thick, and hard for the best effect[*].

But for all ordinary purposes the results for the second and third cases seem most important, and lead to nearly the same

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[*] Weber recommends square bars eight times as long as they are broad, and tempered very hard. (Taylor's Scientific Memoirs, vol. ii. p. 86.)

result ; taking the mean we find for the maximum magnet

$$\frac{b}{d} = \frac{1 \cdot 5}{p}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

We see from all our results that the ratio of the length of a magnet to its diameter in all cases is inversely as the constant p. This constant increases with the hardness of the steel ; and hence the harder the steel the shorter we can make our magnets. It would seem from this that the temper of a steel magnet should not be drawn at all, but the hardest steel used, or at least that in which p was greatest. The only disadvantage in using very hard steel seems to be the difficulty in imparting the magnetism at first ; and this may have led to the practice of drawing the temper ; but now, when we have such powerful electromagnets, it seems as if magnets might be made shorter, thicker, and harder than is the custom. With the relative dimensions of magnets now used, however, hardening might be of little value.

We can also see from all these facts, that if we make a compound magnet of hardened steel plates there will be an advantage in filing more of them together, thus making a thicker magnet than when they are softer. We also observe that as we pile them up the distribution changes in just the way indicated by M. Jamin, the curve becoming less and less steep.

Substituting in the formula the value of p which we have found for Stub's steel not hardened, but still so hard as to rapidly dull a file, we find the best ratio of length to diameter to be $33 \cdot 8$—and for the same steel hardened, about 17, though this last is only a rough approximation. This gives what M. Jamin has called the normal magnet. The ratio should be less for a U-magnet than for a straight one.

For all magnets of the same kind of steel in which the ratio of length to diameter is constant the relative distribution is the same ; and this is not only true for our approximate formula, but would be found so for the exact one.

Thus for the " normal magnet " the distribution becomes

$$\lambda = C(\epsilon^{3\frac{x}{b}} - \epsilon^{-3\frac{x}{b}}),$$

where C is a constant, and x is measured from the centre. The distribution will then be as follows :—

$\frac{x}{b} =$	0.	·1.	·2.	·3.	·4.	·5.
$\frac{\lambda}{C} =$	0	·609	1·27	2·05	3·02	4·26

Fig. 8.
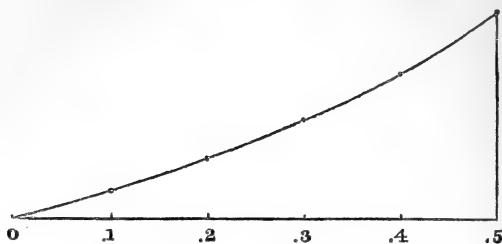
Distribution on "normal magnet."

This distribution is not the same as that given by M. Jamin; but as his method is so defective, and his "normal magnet" so indefinite, the agreement is sufficiently near.

The surface-density at any point of a magnet is

$$\delta = \frac{\mathfrak{H}}{8\pi^2 p \mathrm{R}'} \frac{\epsilon^{2p\frac{x}{d}} - \epsilon^{-2p\frac{x}{d}}}{\epsilon^{p\frac{b}{d}} - \epsilon^{-p\frac{b}{d}}}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (25)$$

which, for the same kind of steel, is dependent only on $\frac{x}{d}$ and $\frac{b}{d}$.

Hence in two similar magnets the surface-density is the same at similar points, the linear density is proportional to the linear dimensions, the surface integral of magnetic induction over half the magnet or across the section is proportional to the surface dimensions of the magnets, and the magnetic moments to the volumes of the magnets. The forces at similar points with regard to the two magnets will then be the same. All these remarks apply to soft iron under induction, provided the inducing force is the same—and hence include Sir William Thomson's well-known law with regard to similar electromagnets; and *they are accurately true notwithstanding the approximate nature of the formula from which they have here been deduced.*

Our theory gives us the means of determining what effect the boring of a hole through the centre of a magnet would have. In this case R' is not much affected, but R is increased. Where the magnet is used merely to affect a compass-needle, we should then see that the hole through the centre has little effect where the magnet is short and thick; but where *it is long, the attraction on the compass-needle is much diminished.* Where the magnet is of the U-form, and is to be used for sustaining weights, the *practice is detrimental, and the sustaining-power is diminished in the same proportion as the sectional area of the magnet.* The only case that I know of where the hole

through the centre is an advantage, is that of the deflecting magnets for determining the intensity of the earth's magnetism, which may be thus made lighter without much diminishing their magnetic moment.

In conclusion, let me express my regret at the imperfection of the theory given in this paper; for although the equations are more general than any yet given, yet still they rest upon two quite incorrect hypotheses; and so, although we have found these formulæ of great use in pursuing our studies on magnetic distribution, yet much remains to be done. A nearer approximation to the true distribution could readily be obtained; but the result would, without doubt, be very complicated, and would not repay us for the trouble.

In this paper, as well as in all others which I have published on the subject of magnetism, my object has not only been to bring forth new results, but also to illustrate Faraday's method of lines of magnetic force, and to show how readily calculations can be made on this system. For this reason many points have been developed at greater length than would otherwise be desirable.

XLII. *On Nodes and Loops in connexion with Chemical Formulæ. By* Oliver J. Lodge, *Demonstrator of Physics in University College, London**.

(1) AT the recent Meeting of the British Association, Professor Cayley communicated to Section B his investigation "On the Analytical Forms called Trees," the principal chemical result of which was the determination of the number of isomeric paraffins for any given number of carbon atoms. He did not deal with any case where the carbon atoms form a closed chain or

cycle as $\begin{array}{c} C\!-\!C \\ \diagdown\;\;C\;\diagup \end{array}$ or $C\!\subset\!\supset\!C$, but only treated of simple ramifica-

tions such as $C\!-\!C\!\!\!\diagup^{\textstyle C}_{\diagdown C\!-\!C}$.

In the discussion after the paper, Professor Clifford said that a paraffin (or hydrocarbon of the form $C_n H_{2n+2}$) could not possibly form a closed chain or cycle of atoms, but must be a simple ramification, and, further, that in a hydrocarbon of the form $C_n H_{2n+2-2x}$ there would always be x cycles—as, for instance, in

benzol, C_6H_6, $12+2-2x=6$, or $x=4$; and
there are 4 cycles, viz. the three double bars and
the whole ring.

(2) I now find that a general expression for
the number of loops * in a compound may be
established with great ease. First of all, the
truth of Clifford's rule is evident; for draw
the graphic formula of C_nH_{2n+2}, and assume
for the present that it has no loops; the H atoms can only be
removed in pairs, and for every pair removed the two liberated
bonds close up, "satisfying each other" and forming one loop.
Hence if $2x$ hydrogen atoms be removed, x loops are obtained,
which is the rule.
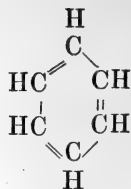
It is also evident that, whether a pair of liberated bonds unite
with an atom of oxygen or with each other, the number of loops
is the same, that an atom of oxygen may be inserted anywhere
in a chain without making a loop, and that a chain of oxygen
atoms can only act as a single dyad radical. Hence the number
of loops in a compound is independent of the amount of oxygen
(or any other dyad element) which it may contain. This proves
that no paraffin can have any loops; for H_2 has none, and they
can all be built up from H_2 by successive additions of CH_2,
which is a dyad radical.

Before proceeding to obtain a general expression for the
number of loops in a compound, it will be well to show by two
examples how the loops are to be counted.

$$B\equiv B \text{ has 2 loops, not 3;}$$

and

$$\text{has 5 loops only.}$$
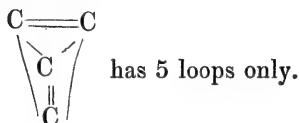
Let us now replace some of the hydrogen in a paraffin by one
atom of a k-ad (denoted by K), so as to form the compound
$C_n KH_{2(n+1-x)+f(k)}$, where $f(k)$ has to be determined so that there
may be x loops.

Now, if K is a dyad, we have seen that the term $f(k)$ does not
appear, or $f(2)=0$.

Also if K is a tetrad, it will act simply as another atom of car-
bon; but another atom of carbon would add 2 to the suffix of
H; therefore the tetrad must add 2, or $f(4)=2$.

These two equations practically give us $f(k)=k-2$; hence
there will be x loops in $C_n KH_{2(n-x)+k}$.

* I use the word *loop* because "*cycle*" seems too good for the purpose.

Example.—Sulphuric acid, $C_0 S^{vi} H_2 O_4$, has two loops, and

$$-2x + 6 = 2 \text{ gives } x = 2.$$

If r atoms of the k-ad are introduced instead of 1, the suffix of H becomes $2n + 2 - 2x + r(k-2)$, and the general law is sufficiently obvious; but it is obtained more satisfactorily in § 8.

Example.—Cyanmethine, $C_6 N_3^v H_9$,

$$12 + 2 - 2x + 3(5-2) = 9 \text{ gives } x = 7 ;$$

and Frankland gives as its formula

$$\text{Me—C}\equiv\text{N—N}\equiv\text{C—Me}$$
$$\text{N}$$
$$|||$$
$$\text{C}$$
$$|$$
$$\text{Me}$$
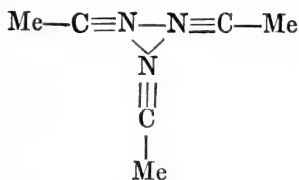
If no hydrogen is present, but only carbon and oxygen,

$$2n + 2 - 2x = 0, \text{ or } x = n + 1.$$

So calcic oxalate, having 2 carbons and some dyads, will have 3 loops.

Heterocline, $Si ('Mn'''_6 O_{11})^{iv}$, containing 7 tetrads, will have 8 loops.

(3) If the formula of a compound be arranged so that some of the bonds cross each other, it is less easy to see how many loops there really are; thus $\begin{smallmatrix} \text{P—P} \\ |\times| \\ \text{P—P} \end{smallmatrix}$ is the same compound as
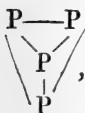
$\begin{smallmatrix} \text{P—P} \\ \diagdown \ \diagup \\ \text{P} \\ | \\ \text{P} \end{smallmatrix}$, and has 3 loops, not 4 as would appear from first

sight. In fact every crossing of the bonds introduces one spurious and unreal loop; because such a crossing may be regarded as a spurious tetrad atom, and each tetrad atom causes one loop. For instance, fig. 1 has 9 apparent loops; but as there are 4 crossings the real number of loops is 5.

Fig. 1.

Apropos of this crossing of the bonds, Professor Vernon Harcourt, President of Section B, raised a question as to the advantage or disadvantage of the system of representing agglomerations of atoms in space by a plane diagram. Now it is evident that any configuration in space may be projected on to a plane surface; and the only difference between such a projection and a diagram drawn directly

on the plane will be, that in the latter the bonds are not usually made to cross each other, whereas in the former they will be very liable to do so.   The benzol chain might be projected out of space as in fig. 2 ; and this form, though probably less true, and certainly less convenient than the usual form, appears to offer certain advantages.   For if it be considered experimentally established that the atoms 2 and 6 are similarly related to 1, then the fact is plainly exhibited in the diagram.   Moreover it will be observed that 2, 4, and 6 are directly united to 1, while 3 and 5 are only indirectly connected ; and I believe that 3 and 5 are considered to possess affinities distinctly differing from the others when 1 is combined, say, with hydroxyl, as in phenol.

Fig. 2.

In counting the loops of the above configuration (fig. 2), it must be noticed that 3 crossings and a loop run together at the middle.   The apparent loops are therefore 7, but $7-3=4$ is the real number.

(4) To investigate the whole subject from a general point of view I shall make use of the fact pointed out above, that whenever two bonds cross, a spurious loop is introduced—but shall no longer consider a crossing as accidental, and its loop as spurious.   State the rule thus :—Every node added to a linkage* made of a fixed number of rods, adds a loop.

Take a loop of string and throw it flat on the table in a tangle ; every node or crossing point accounts for a loop, and there was one loop to begin with, therefore for n nodes there are $n+1$ loops.   Now cut the string in any place, one loop is destroyed, and it becomes a single open curve ; cut it again in another place, it is now two such curves, and another loop is lost.   And generally, if it be made into p such curves p loops will be lost, the number remaining being $n-p+1$.   Or if p open curves be drawn crossing each other or themselves at random on a plane, the number of nodes exceeds the number of loops by $p-1$.   In fig. 3 $p=7$.

Fig. 3.
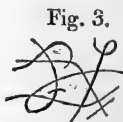
A caution is here necessary: when cutting the string, bits must not be cut out and left to lie wholly detached and not crossing any thing ; for such a piece, though lying close to or inside the tangle, has no more connexion with it than if it lay a mile away.

A jumble of p rods, crossed as above, may be considered as a casement without a frame.   If a simple frame of any shape be fitted to it so that both ends of every rod terminate in the frame, the number of nodes remains n, but $2p$ fresh loops are added.   The whole number of panes in any casement is therefore

* I do not quite know how far it is right to apply this term of Professor Sylvester's to statical combinations of rods having no reference to motion.

$n+p+1$ (fig. 4). If the connexions with the frame Fig. 4.
are counted as nodes, it belongs to the former case.

If the free ends of the jumble of p rods be joined
in pairs, p fresh loops are formed, the figure becomes
a set of closed rings crossing each other, and
the number of its loops (being $n-p+1+p$) exceeds the
number of crossings by 1. Here also no ring must lie without
crossing any thing; every such detached ring causes one loop
too many. It may in fact be said to give only one imaginary
loop for two imaginary crossings, the other loop (viz. itself) being
real.

(5) The maximum number of nodes possible with m *straight*
rods is $\frac{m(m-1)}{2}$. This is evident by constructing a jumble, rod
by rod: the second rod gives one node, the third gives two more,
and the mth gives $(m-1)$ more. Now in any of the above en-
tanglements several lines are apt to cross at one and the same
point; when this happens some loops and nodes are lost. How
many?

Let m lines cross at a single point (such a point may be called
an m-node), the actual number of crossings which there run
together is evidently the maximum number of nodes possible
with m straight rods, viz. $\frac{m(m-1)}{2}$. The number of loops in
the point will be, by the general rule, $m-1$ less or $\frac{(m-1)(m-2)}{2}$.
But the effect of such m-nodes in a tangle may be most readily
allowed for by saying that each is equivalent to $m-1$ ordinary
nodes, the remaining nodes being compensated by vanishing
loops. This is true whether the rods run through the point or
only run into it and stop; so, if one rod runs into another and
stops without crossing it, it must count as an ordinary node. If
a rod terminates in an m-node it adds to it one node more. The
touching point of two curves must be counted as a single node,
no matter how high the order of contact may be. All these things
have followed from the general law that p rods give $p-1$ more
nodes than loops; so that in any figure without loops the num-
ber of nodes must be $p-1$. To any jumble of p open curves or
" rods " any number of closed rings may be added, and the law
of loops and nodes will remain true: but the closed curves must
not, of course, be counted among the p open ones; they may be
entirely neglected.

It can be easily proved that, if the assemblage of ellipses passing
from a straight line through a circle to a straight line cutting
the first at right angles can be inscribed in a square and have x
loops, the number $2x+1$ is a perfect square.

(6) From an m-node $2m$ lines radiate; but an odd number of lines may radiate from those nodes in which a rod terminates without going through. Call these " odd nodes." The smallest number of rods required for the construction of any jumble will be half the whole number of odd nodes in it; for a rod can always pass completely through an even node, but must ultimately stop at an odd node, even though it go through it to begin with.

From this it follows that, since any arrangement of closed curves will have only even nodes, such an arrangement may always be drawn without taking the pen off. The same may be done if a diagram contains two odd nodes; but you must begin at one of them and will leave off at the other. The child's puzzle, fig. 5, since it contains 4 odd nodes, requires 2 rods to make it, and so cannot be drawn without taking the pen off. A Lissajous's figure, being a reentrant curve, has always one loop in excess of the nodes. There is an even number of odd nodes in every tangle.

Fig. 5.

(7) I shall now represent graphic formulæ by arrangements of rods. An ordinary node will represent a tetrad atom, and a node from which k lines radiate, a k-ad atom; odd nodes will be called perissads, and even nodes artiads. Every compound will be supposed to be made of the minimum number of rods, viz. half the number of its perissad atoms. Examples of these formulæ are given in fig. 6, which represents the compounds carbonic anhydride, ethylene, nitric acid, calcic oxalate, benzol, acetic acid, and ferrocyanide of potassium. I consider that such skeleton formulæ may be useful in general investigations, where the atomicity of an element is the principal property to be attended to. They have the advantage of being

Fig. 6.

easy to draw. In fig. 6 the HNO_3 has 1 rod, 2 loops, and 2 nodes; the K_4FeCy_6 has 5 rods. Common alum has only 1 rod, 10 nodes, and 10 loops. A paraffin requires $n+1$ rods.

The nodes in these figures are not to be confounded with a crossing of the bonds; in them, as in other graphic formulæ, the bonds do not cross. An atom exists at every node; and its atomicity, which is exhibited, is in general sufficient to give its name. Dyad atoms are like singular points or knots on the string of § 4. (Professor Cayley's *tree* "*knots*" are tetrad atoms.) There is no necessity for dotting in the hydrogen atoms; free bonds are best represented by arrow-heads.

(8) We have proved (§§ 4 and 6) that, calling P the number

of perissad atoms in a compound, the number of nodes (n) exceeds the number of loops (x) by

$$\frac{P}{2} - 1,$$

or

$$x = n - \frac{P}{2} + 1.$$

But since triad and tetrad atoms count as ordinary nodes, while pentads and hexads count as double nodes (§ 5),

$$n = (\text{III}) + (\text{IV}) + 2(\text{V}) + 2(\text{VI}) + \ldots,$$

where (III) denotes symbolically the number of triad atoms in the compound, (IV) the number of tetrads, and so on; hence

$$x = 1 - \frac{P}{2} + (\text{III}) + (\text{IV}) + 2(\text{V}) + \ldots,$$

or

$$x = \frac{P}{2} + 1 - (\text{I}) + (\text{IV}) + (\text{V}) + 2(\text{VI}) + \ldots,$$

or

$$2x = 2 - (\text{I}) + (\text{III}) + 2(\text{IV}) + \ldots = 2 + \Sigma(r \cdot \overline{k-2}).$$

Each of these expressions has its convenience; the last tells us that if there are r atoms of atomicity k, s atoms of atomicity l, &c. (that is, in the compound $K_r L_s \ldots$), the number of loops is

$$\frac{r(k-2)}{2} + \frac{s(l-2)}{2} + \ldots + 1.$$

(9) This general theorem shall now be applied to a special case. Suppose L is a monad like hydrogen, $l = 1$, and the number of loops in $K_n H_m$ is $\dfrac{n(k-2)}{2} - \dfrac{m}{2} + 1$. Calling this number x, it is evident that $m = n(k-2) + 2 - 2x$, or that there are x loops in the compound $K_n H_{n(k-2)+2-2x}$. If now K be a tetrad (such as carbon), we shall have x loops in $C_n H_{2n+2-2x}$, which is Clifford's rule (§ 1). It also follows that the only hydrocarbon with no loops is $C_n H_{2n+2}$; and more generally, that if an h-dro-c-bon with n-atoms of atomicity c has no loops, it must contain $\dfrac{n(c-2)+2}{2-h}$ atoms of atomicity h. This shows that in binary linkages with no loops one of the elements must be a monad, while the other may have any atomicity whatever.

(10) A simple way of proving the general formula (§ 8) for the number of loops in a compound $K_r L_s \ldots$ consists in taking a ring of any number of dyads, turning them into k-ads by adding

$k-2$ bonds to each, and joining the bonds in pairs; every pair of bonds adds a loop to the figure, and there was the original ring to start with.

Since the number of loops in a compound is determined by its empirical formula, or number of atoms, it is evidently independent of the arrangement of those atoms; consequently *all isomers have the same number of loops in their composition.* Also if any graphic formula has no loops in it, it cannot be made to have any by any rearrangement of the atoms. But it must be understood that the loops formed by "latent" bonds are to be counted; otherwise one kind of ethylic cyanide, for instance, would have two loops more than the other.

(11) Several of the formulæ are apt to give a negative number of loops in certain cases. There is a certain amount of meaning in this. For instance, a single monad molecule has no loops and no nodes; two detached monad molecules must be considered as having no nodes and -1 loop; if their bonds are laid across each other you get 1 node; and (§ 3) the node introduces a loop, raising the number of loops to 0 (v. fig. 7). If one atom of each molecule in the crossing pair be made a dyad, a loop is formed; not so with the detached pair. It is true that if both the atoms of a single molecule be made dyads a loop is obtained; but the detached molecule must be considered as subtracting a loop, leaving 0 as the total number. In fact with n detached simple molecules the number of loops is $1-n$. Three detached hydrogen molecules have -2 loops; to make one loop with them you have to cross the bonds three times. Of course the real number of loops is not changed by any amount of crossing, those introduced by the nodes are what I formerly (§ 3) called spurious ones. The number of nodes appears incapable of being negative, except under the imaginary circumstances of § 13.

Fig. 7.

(12) It will now be seen that detached rods or rings (§ 4) may be considered as having *some* connexion with our entanglements, and may be taken into consideration by means of the convention just stated; but a former statement remains true, that the connexion does not vary with the distance.

Water of crystallization may be said to lie in detached simple molecules; and if the number of loops in a crystallized compound be determined from its complete formula instead of from that of the anhydrous salt, the number obtained will not be erroneous if one is willing to consider that every detached water molecule corresponds to a negative loop. The same is true of any kind of molecular combination other than that usually represented by bonds, as in double salts. A further consequence of § 11

is that in the equation of a chemical reaction where a compounds on one side are equated to b compounds on the other, and where x is the sum of the number of loops of the a compounds, there the sum of the number of loops of the b compounds is

$$x - a + b.$$

With this result the number of loops in any compound may be immediately found by equating it to all its elements; and this indeed is the most direct way of establishing the general formula of § 8.

(13) We found in § 11 that every detached compound corresponds to a negative loop; hence a single compound (being detached) ought strictly to be considered as giving a negative loop; or the real number of loops in a compound is in all cases one less than the number which we have so far been satisfied with, and which all our expressions give.

Thus in a linkage of r atoms of a k-ad there are $\dfrac{r(k-2)}{2} + 1$ loops; therefore in no atoms of an any-ad there is 1 loop—which would have seemed absurd, only the reason is now plain, viz. that the presence of a compound is itself a negative loop, and so its removal adds a loop. The presence of the 1 in all our formulæ is thus explained. Taking the negative loop of presence into account, it disappears; and then no atoms of a k-ad give no loops.

Further, there is no reason why we should limit ourselves to atoms of positive atomicity; in fact we have not done so; for we shall see that monads are really negative. Thus, take a homogeneous linkage of k-ads, represent number of loops as ordinates, and number of atoms as abscissæ; the curve is a straight line for every value of k, but it has different slopes. It makes 45° with the axes for $k=4$ or tetrads, and again 45° in the other quadrants for $k=0$ or no-ads, while for $k=2$ the line is parallel to the axis of x. Hence 2 is the natural zero of atomicity (at any rate in "loop"-considerations), not 0. If this natural zero were used instead of the ordinary one, and if further the "presence-loop" were taken into account, all the expressions would simplify and become more symmetrical. Every atom would then give a number of loops equal to half its natural atomicity, and the general formula of § 8 would become

$$x = \tfrac{1}{2}\Sigma(kr).$$

Thus, then, monads have negative bonds, and subtract loops instead of adding them. Whatever a $(2+k)$-ad does, a $(2-k)$-ad can undo; *e. g.* every tetrad adds a loop, every no-ad subtracts one, and so on.

Moreover there is no necessity for limiting our statements to

satisfied * compounds as hitherto; they apply equally well to radicals with free bonds, every free bond being counted as half a loop. (This is justified either by experiment with the formulæ, or by remarking that 2 free bonds join together to form 1 loop.) A (-1)-ad gives $1\frac{1}{2}$ negative loop. The removal of atoms of any atomicity less than 2 increases the number of loops. If we replace m atoms of hydrogen in a paraffin by r atoms of a k-ad, we introduce $\frac{m+rk}{2} - r$ loops, $\frac{m}{2}$ by the removal of the H, the rest by the ordinary action of a k-ad.

The change of zero seems unnatural if the atoms are considered individually and separate; but then they do not naturally exist in that condition. If atoms be added to a line diagram ($\S 7$), the meaning of the change is evident†: triads require one bond to be drawn, dyads can be put in anywhere; monads usually necessitate some rubbing out, and always produce an abrupt termination. If I may be allowed to express myself rather wildly, it is as if the normal condition of an atom were to have two attractive centres, and as if a monad were in a stunted condition. Might I suggest the term *order* for non-chemical use, instead of *atomicity* or *valency*, which, though doubtless they do very well in chemistry, are not pleasant words? The change of zero might be made at the same time, and atoms of tetravalent atomicity be called atoms of the 2nd order, and so on. The word has the advantage of an analogous signification in geometry.

University College,
September 20, 1875.

* The only thoroughly *saturated* compounds are the loopless ones, for wherever there is a loop there is the possibility of its opening out and taking a monad on each end; thus starch $(C_6 H_{10} O_5)_z$ has $z+1$ loops, and might therefore take up $2z+2$ monad atoms before being fully saturated.

† It may be noticed that while dyad atoms can build up only dyad radicals, and artiad atoms only artiad radicals, triad atoms can build up radicals of any atomicity whatever, and hence that, if the elements are composed of some fundamental substance, the simplest supposition to make concerning the atoms of that substance is, that they are triads, or atoms of the first order. In the article on Classification in Watts's 'Dictionary of Chemistry,' Professor Foster has calculated the maximum free atomicity of a compound, and points out that additional dyads do not increase it at all; triads increase it by 1, tetrads by 2, and so on; which is similar to what I am saying above.

XLIII. *On Stationary Liquid Waves.*
By FREDERICK GUTHRIE.
[Concluded from p. 302.]

§ 20. *MONONODAL undulations in rectangular troughs.*—
In rectangular troughs the mononodal wave-system
can be established by tilting or by the use of a wide stirrer.
The wave-length is twice that of the trough. As the shallowness
influences longer waves more than shorter ones, we must look
for greater discrepancies here than in §§ 15, 16, 17. Taking
the same troughs, namely Z, Y, X, W, the first experiments were
to determine the influence of depth.

Influence of Depth on Mononodal Waves in
Rectangular Troughs.

Depth.	Z.	Y.	X.	W.
millims.				
120	......	......	......	92
140	......	......	......	93
160	......	......	......	94
180	......	......	......	94
200	49	......	......	94
220	51·5	......	......	94
240	53	61	74·6	94
260	54	62·2	75·2	94
280	55	63	76	94
300	55	63	76	94

Experiments with Z (depth = 290 millims.).

In 2′ there were 110 waves.
„ 3 „ 165 „
Mean rate for Z per 1′ . . . 55

Experiments with Y.

In 3′ there were 189 waves.
„ 2′ „ 126 „
Mean rate for Y per 1′ . . . 63

Experiments with X.

In 3′ there were 228 waves.
„ 2′ „ 152 „
Mean rate for X per 1′ . . . 76

Experiments with W.

In 1′ there were 94 waves.
„ 1′ „ 94 „
„ 1′ „ 94 „
Mean rate for W per 1′ . . . 94

Collecting and multiplying each number of waves by the square root of its trough-length (trough-length $= \frac{1}{2}$ wave-length, the ratio is the same), and dividing all such products by the least of them, we get the following numbers:—

	Length of trough. l.	Mean number of waves in $1' = n$.	$n\sqrt{l}$.	
Z . . .	767	55	1523·21	1·00
Y . . .	619	63	1568·42	1·03
X . .	463	76	1635·32	1·07
W . .	308	94	1649·70	1·08

In spite, therefore, of the apparent attainment of maximum rate at 280 millims., we must reject the two longest waves in adopting a constant, which, taking the mean of the two shorter systems, is 1642·5.

In all the four cases, namely mono- and binodal waves in circular and rectangular troughs, the value of $n\sqrt{l}$ or $n\sqrt{d}$ increases as the length of l or d decreases. The increase of rate of wave progress due to increased wave-length is somewhat more than counterbalanced by the increased path, so that the "constant" is larger with smaller troughs.

§ 21. *Comparison between mononodal and binodal waves in rectangular troughs*.—In the same trough the wave-length of the mononodal system is always that of the binodal system, the former being $2l$ ($l =$ trough-length). If the ratio of progression be as the square root of the wave-length, then the number n of waves in a given time, varying directly as the rate of progression and inversely as the distance, in the same trough

$$\frac{\text{number of binodal}}{\text{number of mononodal}} = \sqrt{2}.$$

How far this is borne out in the experiments is seen in the following comparison of the shorter troughs, where the mononodal system is normal.

	Length of trough.	$\sqrt{2} \times$ mononodal.	No. of binodal.
X . . .	463	107·47	110·4
W . .	308	132·94	135·8

§ 22. If we take a binodal system and insert a rigid plane diaphragm vertically down the middle and at right angles to the plane of the wave, we divide it under little disturbance obviously into two mononodal systems of half the trough-length, but of the same wave-length as before. These should oscillate (inde-

pendently of one another) at the same rate and at the rate of the original system. The experiment which illustrates this relation most directly is $Y(l = 619$ millims.) of the binodal, where $n = 94.5$, and W $(l = 308)$ of the mononodal, where $n = 94.0$. Such a material diaphragm cannot satisfy the ideal conditions; for if at rest, additional friction is introduced by the motion along its surfaces of pairs of particles at each side exerting equal and opposite pressures. These,

Fig. 4.

in the absence of the diaphragm, exert no friction on one another, since they move at the same rate.
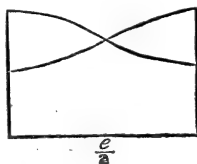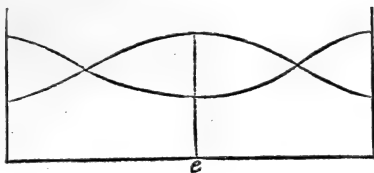
§ 23. *General comparison with Pendula.*—As the amplitude of the wave produces no sensible effect on its rate of progression, and the latter varies for small wave-lengths directly as the square root of the wave-length both in circular and rectangular troughs, the oscillation of water may be closely compared with that of a pendulum, which for small circular arcs is isochronous, whatever be the amplitude of displacement; height of wave : height of pendulum-bob $= v(1 - \cos \frac{1}{2}\theta)$. The mean velocity of the pendulum-bob varies as the arc for the same radius; for the same small angular displacement inversely as the square root of the radius. We have then the analogous cases :—

Pendulum.	Water-wave.
$n \sim \dfrac{\text{velocity}}{\text{path}},$	$n \sim \dfrac{\text{velocity}}{\text{path}},$
$\sim \dfrac{\sqrt{r}}{r},$	$\sim \dfrac{\sqrt{l}}{l},$
$\sim \dfrac{1}{\sqrt{r}},$	$\sim \dfrac{1}{\sqrt{l}}.$

The moving force in a simple pendulum is always equal to its weight, though this force is always applied partly to the point of suspension. In the moving water the to-and-fro motion is due to the action of a variable difference of pressures (heights). The law of oscillation, however, is preserved, although the undulations are the result of particle-motion in the mass of the liquid, and although these particles in the case of stationary

waves, as was pointed out so many years ago by the brothers Weber, do not complete their orbital motion, but themselves swing, like the bobs of elementary pendula, to and fro in short-arc'd rhythmic sequence. Whenever the momentum and inertia vary together (as in all cases of solid magnification—*i. e.* alteration of size, conservation of shape, and either conservation or alteration of density), the pendular law must be preserved, and this whether the mass to be moved is wholly active, as in the pendulum itself, or more or less passive, as the logan oscillation of a common balance.

In rectangular troughs it is easily shown that, if the surface is plane, the pendulum (elasticity) or torsion law must hold good, and that the force of restoration is proportional to the linear measure of disturbance. If A B (fig. 5) be the level of

Fig. 5.
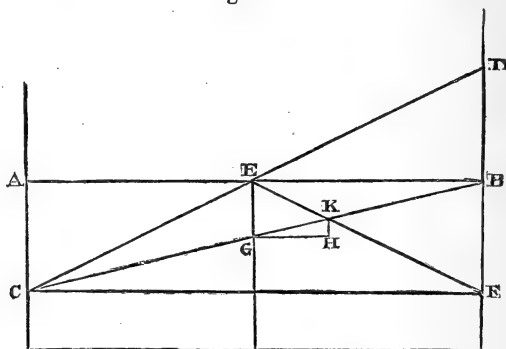
the water in a rectangular trough and it is set in mononodal undulation, so that in one phase the surface is at C E D, the line B D, B F, or A C is half the amplitude. Consider all the water below C F to be without influence. The rectangle of water A C B F has become the triangle C F D. Join C B and E F; their intersection K is the centre of gravity of C D F, and is $\frac{2}{3}$ of C B from C, &c. Draw G H horizontal and K H vertical till they intersect. The centre of gravity of the rectangle A C B F has been raised by the change of form from G to K—that is, from H to K. The work in the displaced water is D C F × K H.

$$KH = KG \frac{BF}{BC},$$

and

$$KG = (\tfrac{2}{3} - \tfrac{1}{2})BC = \tfrac{1}{6}BC,$$

or

$$KH = \tfrac{1}{12}DF.$$

The work done and potential at maximum excursion vary directly with displacement. What is true for a plane is of course true for a rectangular trough. It can also be shown to be true for disturbances in circular troughs from fig. 3, § 11, and follows indeed without further proof from the equal tetrahedron and wedge having been generated by the rotation of a triangle balanced about one of its lines of gravity. The fact that the surface of both wave-systems is curved, and that this curve involves the cohesion and viscosity of the liquid, puts the exact solution of the question beyond the present power of analysis.

§ 24. *Absolute comparison of circular waves with pendulum.*— It appears that both the binodal waves in circular troughs and the binodal waves in rectangular troughs follow the pendular law, and that the mononodal waves in rectangular and circular troughs do so nearly. It follows from § 23, where it is shown that no dead weight has to be stirred, that, if the liquid has no viscosity, not only should the pendular or torsion law hold good, but the actual rate of undulation in the binodal system in the unhampered or circular wave should be the same as that of a pendulum of radius equal to half the wave-length— that is, to the diameter of the cylinder. If a pendulum of length l be suspended over a circular trough of radius l, it keeps time very nearly with the fundamental undulations in the trough. This can be beautifully shown in an experiment where the pendulum-length can be altered till it is isochronous with the pulsations. But it appears best from the data already given. Taking the length of the seconds-pendulum at London at 994 millims., the number of oscillations which pendula of the radii of the circular troughs give is shown in the following Table,

$$n' = 60 \sqrt{\frac{994}{r}}.$$

	(1) Radius of trough, r.	Number of liquid undulations.	(n'). Number of oscillations of pendulum of length r.
A	297·5	106·9	108·6
B	228	122·4	125·3
C	183	136·7	139·4
D	157	149·0	150·9
E	150	151	154·4

The pendulum-oscillations exceed the liquid ones, on an average, 2·8; on the corrected average, under the omission of E, 2·05. The mean per-cent. deficit is about 1·5. Considering that the liquid undulations do not, even in the largest troughs, sustain themselves for more than 20′, while a pendulum

in air will oscillate for hours, and remembering that rate of de-
cadence is nearly proportional to individual delay, I do not think
that the above numbers are at all in disaccord with the asser-
tion that the *fundamental or binodal circular undulations in an
infinitely deep circular vessel are isochronous with those of a pen-
dulum whose length is equal to the radius of the vessel.*

In hemispherical and cycloidal vessels I anticipate that a
still nearer identity would be obtained. The exact form of the
cup of revolution which would give the nearest approach to the
pendulum-rate depends of course upon the shape of the wave;
and here we are again referred to the complex influence of vis-
cosity and cohesion.

§ 25. *Comparison of rectangular troughs with pendulum.*—
Making a similar comparison to the above with rectangular
troughs having binodal undulations, let us find what are the
lengths of pendula isochronous with the water-waves in the
troughs Z, Y, X, W respectively. The number of liquid undu-
lations being *n*, the formula

$$l = \left(\frac{60}{n}\right)^2 994$$

gives us the required lengths of the pendula in millims.

	Trough- or wave-length.	Number of liquid un- dulations.	Length of isochronous pendulum.	$\frac{2}{\pi} \times$ trough- length.
Z	767	84·5	501·194	488·4
Y	619	94·5	400·482	394·1
X	463	110·4	293·627	294·8
W.........	308	135·8	194·028	196·1

It appears, therefore, that *the binodal undulations in a rectan-
gular trough are isochronous with the oscillations of a pendulum
whose length is* $\frac{2}{\pi}$ *of the trough or wave-length.*

§ 26. *Comparison between circular and rectangular troughs in
binodal vibration.*—Briefly, it has appeared that the differences
between the wave-systems in circular and rectangular troughs,
both being binodal, are as follows :—In rectangular troughs the
nodes are one fourth of the trough-length from the ends. In cir-
cular troughs they are one sixth of the diameter. In rectangular
troughs the amplitude is a very little more at the centre than at
the edges. In circular troughs it is very little more than double
that at the edges. The rate of progression in both varies as the
square root of the wave-length; and the rate of sequence, or *n*,
varies inversely as the square root of the wave-length. The
waves of the circular system are isochronous with a pendulum

of half the wave-length; in the rectangular trough this pendulum must be $\frac{2}{\pi} \times$ wave-length. If reflection does not alter velocity, the circular wave 1 metre long will travel 83·1 metres in one minute if it preserves its wave-length; while a wave 1 metre long between walls will only travel 74·7 metres in one minute. In answering the question as to whence this latter difference arises, a few preliminary experiments are useful. That waves in circular troughs endure longer than waves of the same amplitude in rectangular ones may be attributed in great measure to friction. That element of friction which is due to adhesion and cohesion can be directly compared as follows. In a square trough in one complete undulation the surface rubbed by the two end lines of particles is $4al$ if a is the amplitude and l the trough or wave-length. The surface rubbed by the two side-lines of particles is $2al$. The whole surface rubbed by the four edges of the top layer of particles is $6al$. In the circular trough whose diameter is equal to l, the amplitude at the centre being a, the surface rubbed by the edge ring of particles in a complete undulation is $2a\pi\frac{1}{2}l$ or $a\pi l$; or the cohesive friction on the square trough is for the upper layer 1·9 times that of the circular trough for the same wave-length and edge-wave amplitude. If such friction were the cause of the slower rate of wave-progress in rectangular troughs, we should expect the effect to be less in wider troughs than in narrower ones, because, while the surface scraped would be the same, the mass of the water would be greater.

Accordingly the rectangular troughs W, X, Z, which have the dimensions

				Width.	Length.
W	.	.	.	320	308
X	.	.	.	320	463
Z	.	.	.	322	767,

were filled to a depth of 280 millims., and by means of long laths set in binodal undulation in the direction of their widths. The wave-lengths of W and X were accordingly identical; and the wave-length of Z was only 4 millims. longer. The mean of four concordant determinations in each case showed in $1'$,

for W	.	.	.	134
,, X	.	.	.	134
,, Z	.	.	.	133·5

In brief, the length of the trough *across* which the system

Fig. 6.

was established produced no sensible variation in rate of phase-recurrence, and therefore presumably none in wave-progression.

But a wave in a rectangular trough undergoes a constraint quite other than that of friction. Its motion is repressed by the sides. It presses equally and radially in all directions. The wall offers resistance perpendicular to itself. The resultant of the quadrant of the forces in fig. 6 makes an angle of 45° with the wall. The lateral pressure on the wall is $\frac{1}{\sqrt{2}}$ of the total radial pressure; the onward pressure is also equal to $\frac{1}{\sqrt{2}}$, the total pressure. The constant obstruction thus offered by the walls is the cause of the preservation of amplitude. The constant obstruction thus offered by the rigid parallel walls, while preserving amplitude, is, I imagine, the main cause of the lesser rate of wave-progress in rectangular troughs; and could exact measurement be effected, I should expect that the ratio between the times of subsidence from one amplitude to a lower one in a circular and rectangular trough would be found to be nearly the ratio of 83·1 to 74·7. The measurements of rate of subsidence cannot be made of sufficient exactness to test this experimentally; but it is clear enough that binodal waves in circular troughs are far more enduring than those of rectangular ones.
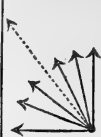
§ 27. It may be shown that in rectangular troughs, when mononodal undulation takes place, the path of the centre of gravity is a parabolic arc, supposing the surface to remain flat. But as this condition never obtains in reality, and as no one particle remains in its motion the centre of gravity of the moving mass, this parabolic motion of the centre of gravity is of no direct account.

§ 28. *Cross binodal undulations in circular troughs.*—On pressing simultaneously two opposite points of the rims of A and B, they yield sufficiently to start undulations which are analogous to the ring of a bell or the fundamental quadrant vibration of a Chladni plate. By repeating the push at proper intervals, a well-shaped system of undulation is established, consisting of quadrant waves separated by diametral nodes at right angles to one another. The rate of recurrence of these in troughs A and B was examined. The troughs were filled with 300 millims. of water.

Number of recurrences
of same phase in 1'.

$$
\text{A.} \begin{cases} 94 \\ 94 \\ 94 \end{cases}
$$

$$
\text{B.} \begin{cases} 109 \\ 109 \\ 109 \end{cases}
$$

Taking the troughs A and B, we have the following comparison :—

Fig. 7.


```
A....106·9        71·6              94
B....122·4        83·5             109
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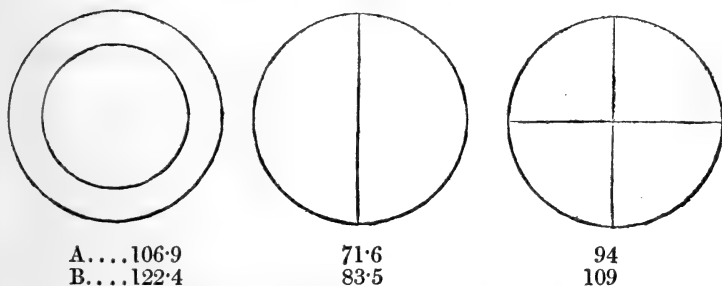
§ 29. *Compound motions.*—The motion given to a glass of water when the axis is made to describe a narrow cylinder and then brought to rest, whereby a wave careers round the vessel, is a modification of the mononodal. If the node of such a system performs one complete revolution during the phase-life of the mononodal wave, the crest reappears in the same place. In the trough B, which held 300 millims. of water, in each of three experiments the crest reappeared in the same place eighty-four times in one minute. The travelling of the node, therefore, makes no difference in the wave-rate. The motion of the surface resembles that of a disk which is at right angles to an axis when the axis describes a circular cone.

If the water revolves on its axis in the trough, mononodal undulations are carried round in a similar manner.

The revolution of the water does not interfere with the period of the binodal circular waves. Thus the water in A, being 300 millims. deep, was stirred round; and while in rapid rotation, it was impressed with the circular binodal wave-system. In each of two minutes 107 waves were counted; and the last minute was immediately following the first. At the end of the experiment the water was still circulating at the rate of 12° per 1″.

§ 30. *Oil on water.*—The restraining effect of a layer of oil

on the water, inasmuch as it causes the more rapid loss of amplitude, might be supposed to effect a retardation in the wave's progress.   Such is not the case, apparently ; for a layer of olive-oil 5 millims. in thickness undulated with the water beneath it in the trough B at such a rate as to give in three experiments the numbers 123, 122·5, 122·5.

§ 31. *Viscosity.*—Glue added in varying quantity up to 1 lb. of the solid in the trough B did not change the rate from 123.

§ 32. *Images on wall.*—It can readily be imagined that the sun shining on water or, better, water covered with oil in one of the states of motion above described, gives most elegant figures on the wall.   The circular binodal, for instance, shows lateral and vertical loop-exchange; the cross binodal circular shows a collapsing and expanding parallelogram ; the binodal rectangular exhibits bars of light which approach and recede with a pause and slight regression.   From the distribution of light in the images, the kind and, to some extent, the degree of curvature can be ascertained.   Thus, when the circular binodal has nearly subsided, the only alteration visible is the change of luminosity in the centre, due to the alternating convexity and concavity.

§ 33. *Recent contributions to the subject.*—On the occasion of the communication of my experiments to the Physical Society, I invited the Members to offer suggestions regarding the *rationale* of some of the stationary waves which were there exhibited.

Mr. O. Lodge has been good enough to point out how completely the phenomenon depends upon the relationship between wave-length and rate of wave-progress.   I regard the results as being of value, in a great measure because they show, I believe, more conclusively than can be shown by the wave-progress on open sheets of water, that this law, that the velocity of progress is directly proportional to the square root of the wave-length, is a true one.   This they do by reason of the conservation of wave-length.

In 'Nature' of July 28th Lord Rayleigh discusses the theoretical aspect of the question, and shows that the wave-rate of sequence which I had given for circular binodal waves corresponds closely with the theoretical number deduced from the hypothesis that in troughs of infinite depths the variation of the diameter is the only thing which affects the wave-rate or period. That the pendulum-, or, as I should prefer to call it, the Logan-law, or law of magnification, should be so closely preserved through the complex molecular movement which actually takes place is of the greatest interest—the more so as the paths of the particles are not increased by an increase of the trough, but there is only a diminution of the eccentricity of their

fragmentary orbits. This has, of course, its counterpart in vibrating elastic rods. The mathematical interpretation of the experimental fact is very valuable. I do not suppose that such interpretation can be complete until account is taken of the cohesion and viscosity of liquids, to which, on the other hand, such experiments may supply a new weapon of attack. The other kinds of wave-systems, the rate of which Lord Rayleigh predicts mathematically, and which in rectangular troughs were described, but not investigated with regard to their period, in the elaborate work of the brothers Weber, I had already examined in regard to their period both in circular and rectangular troughs. In this respect it is safe to predict that every system of vibration of a square or round Chladni plate may be reproduced in a square or round vessel of water, and that the time-ratio will be preserved. Further, in water-wave systems we can have mononodal vibration, which for automatic motion is inadmissible in vibrating plates. With the exception of those above studied, the more complex systems appear short-lived.

34. That the wave-motion of the binodal circular system is not only isochronous with the radius pendulum, but also coincides with pendular motion throughout the whole course of both, appears from the fact that the two sustain one another when in direct integral mechanical connexion. Fig. 8 shows such an arrangement. A tuning-fork, B, having the plane of

Fig. 8.
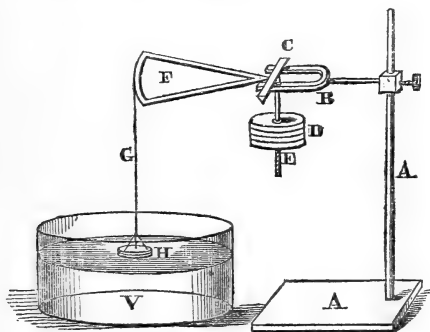
its prongs horizontal, is fastened to a heavy stand, A. Resting on the fork is a knife-edge, C. To C is attached a vertical screw-rod, E, carrying a nut (invisible in the figure) which supports the heavy weight D, consisting of a series of leaden disks. Also fastened to C is a cardboard sector, F. The circular edge of F is split, and into the crack is pasted a paper gutte. A silk thread, G, is fastened to F, and carries a little paraffin disk, H. The pendulum-length is altered by the nut E until the disk H

is isochronous in its motion with the water in V. The vessel V is shown out of proportion small. When the right length of E is obtained, the disk H moves up and down with the water without a ripple for twenty minutes or more, showing how truly pendular is the wave-motion. It follows that, for small disturbances, the rate of vertical motion at the normal level must be closely proportional to the altitude of the wave or amplitude of undulation.

XLIV. *General Integration of* Laplace's *Differential Equation of the Tides.* *By* Sir WILLIAM THOMSON, *F.R.S.**

1. LAPLACE considers the ocean as a rotating mass of frictionless incompressible liquid covering a rotating rigid spheroid to a depth everywhere infinitely small in proportion to the radius, and investigates its oscillations under the influence of periodic disturbing forces, with the limitation that the rise and fall is nowhere more than an infinitely small fraction of the depth, the condition that the mean angular velocity of every part of the liquid is the same as that of the solid, and the assumption that the distance from summit to summit of the disturbed water-surface is nowhere less than a large multiple of the depth. This last assumption is, though not explicitly stated by Laplace, implied in, and is virtually equivalent to, his assumptions (*Mécanique Céleste*, Livre I. No. 36) that the vertical motion of the water is small in comparison with its horizontal motion, and that the horizontal motion is sensibly the same for all depths.

2. Let now h be the elevation of the water-surface above mean level, and ξ and $\eta \sin \theta$ the southward and eastward horizontal component displacements of the water at time t, and at the place whose north latitude is $\frac{\pi}{2} - \theta$ (or north-polar distance θ) and east longitude ψ. The "equation of continuity" [*Méc. Cél.* Liv. I. No. 36, or Airy, "Tides and Waves" (*Encyclopædia Metropolitana*), art. (72)], is

$$\frac{d(\gamma\xi)}{d\theta} + \frac{\gamma\xi \cos\theta}{\sin\theta} + \frac{d(\gamma\eta)}{d\psi} + h = 0, \quad . \quad . \quad . \quad (1)$$

or

$$\frac{d(\gamma\xi \sin\theta)}{\sin\theta \, d\theta} + \frac{d(\gamma\eta)}{d\psi} + h = 0, \quad . \quad . \quad . \quad (1) \, bis$$

where γ denotes the ratio of the depth of the sea to the earth's radius. And the dynamical equations [*Méc. Cél.* Liv. I. No.

36 (M), Airy (87)], are

$$\left.\begin{array}{l} \dfrac{d^2\xi}{dt^2} - 2n\sin\theta\cos\theta\dfrac{d\eta}{dt} = -\dfrac{g}{r^2}\dfrac{d(h-e)}{d\theta}, \\[2mm] \sin^2\theta\dfrac{d^2\eta}{dt^2} + 2n\sin\theta\cos\theta\dfrac{d\xi}{dt} = -\dfrac{g}{r^2}\dfrac{d(h-e)}{d\psi}, \end{array}\right\} \quad . \ (2)$$

where r denotes the earth's radius, n the angular velocity of its rotation, g the force of gravity at its surface, and e the "equilibrium tide-height" at time t, and colatitude and longitude θ and ϕ; that is to say [Thomson and Tait's 'Natural Philosophy,' § 805], the height at which the water would stand above the mean level if it were so placed at rest relatively to the rotating solid that it would remain at rest if the disturbing force were kept constantly what it is in reality at time t.

3. Laplace remarks that the general integration of these equations presents great difficulties; and he confines himself to a very extensive case, that in which γ is a function of latitude simply, and is the same in all longitudes. In this case the complete integration is to be effected by assuming

$$\left.\begin{array}{l} h = \mathrm{H}\cos(\sigma t + s\psi), \\ \xi = a\cos(\sigma t + s\psi), \\ \eta = b\sin(\sigma t + s\psi), \end{array}\right\} \quad . \ . \ . \ . \ . \ (3)$$

provided the disturbing force is such that

$$e = \mathrm{E}\cos(\sigma t + s\psi), \quad . \ . \ . \ . \ . \ . \ (4)$$

where $\mathrm{H}, a, b, \mathrm{E}$ are functions of the latitude, of which E is given, and H, a, b are to be found by integration of the equations. With this assumption (1) *bis* and (2) give

$$\dfrac{d(\gamma a\sin\theta)}{\sin\theta\, d\theta} + s\gamma b + \mathrm{H} = 0, \ . \ . \ . \ . \ . \ . \ . \ (5)$$

$$\left.\begin{array}{l} \sigma^2 a + 2n\sigma\sin\theta\cos\theta\, b = \dfrac{g}{r}\dfrac{d(\mathrm{H}-\mathrm{E})}{d\theta}, \\[2mm] \sigma^2 b + 2n\sigma\dfrac{\cos\theta}{\sin\theta}a \ = -\dfrac{g}{r}\dfrac{s(\mathrm{H}-\mathrm{E})}{\sin^2\theta}. \end{array}\right\} \quad . \ . \ . \ (6)$$

Putting, in these,

$$\mathrm{H} - \mathrm{E} = u, \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ . \ (7)$$

we find

$$\left.\begin{array}{l} a = \dfrac{g}{r}\dfrac{\dfrac{du}{d\theta} + \dfrac{2ns}{\sigma}\dfrac{\cos\theta}{\sin\theta}u}{\sigma^2 - 4n^2\cos^2\theta}, \\[5mm] b = -\dfrac{g}{r}\dfrac{\dfrac{2n}{\sigma}\dfrac{\cos\theta}{\sin\theta}\dfrac{du}{d\theta} + \dfrac{su}{\sin^2\theta}}{\sigma^2 - 4n^2\cos^2\theta}, \end{array}\right\} \quad . \ . \ . \ . \ (8)$$

and then, eliminating a, b, H from (5), (7), (8),

$$\frac{g}{r}\left\{\frac{d}{\sin\theta\,d\theta}\frac{\gamma\left(\sin\theta\frac{du}{d\theta}+s\frac{2n}{\sigma}\cos\theta\,u\right)}{\sigma^2-4n^2\cos^2\theta}\right.$$
$$\left.-\frac{s\gamma\left(\frac{2n}{\sigma}\frac{\cos\theta}{\sin\theta}\frac{du}{d\theta}+\frac{su}{\sin^2\theta}\right)}{\sigma^2-4n^2\cos^2\theta}\right\}+u=-\text{E}. \quad (9)$$

This is Laplace's differential equation of the tides [*Mécanique Céleste*, Liv. IV. No. 3, equation (4); or Airy, "Tides and Waves," *Encyclopædia Metropolitana*, art. (95)]. It is a linear differential equation of the second order, the complete integration of which gives u, and thence, by (8), a and b, in terms of θ, with two arbitrary constants to be determined so as to fulfil proper terminal conditions ($\S\S$ 11–17, below). It is essentially in the form in which Airy gave it, being that in which it comes direct from the formulæ preceding it in the investigation. It originally appeared in the *Mécanique Céleste*, masked somewhat by the addition and subtraction of a certain term which gives it a different form, not seeming at first sight better or simpler; but this as it were capricious modification suggests the following very substantial simplification.

4. Put

$$(\sin\theta)^{\frac{2ns}{\sigma}}u=\phi$$

and

$$(\sin\theta)^{\frac{2ns}{\sigma}}\text{E}=\Phi\,; \qquad\qquad (10)$$

then we have

$$a=\frac{g}{r}(\sin\theta)^{-\frac{2ns}{\sigma}+1}\frac{\frac{d\phi}{\sin\theta\,d\theta}}{\sigma^2-4n^2\cos^2\theta},$$

$$b=-\frac{g}{r}(\sin\theta)^{-\frac{2ns}{\sigma}}\left\{\frac{\frac{2n}{\sigma}\cos\theta\frac{d\phi}{\sin\theta\,d\theta}}{\sigma^2-4n^2\cos^2\theta}+\frac{s\phi}{\sigma^2\sin^2\theta}\right\}. \qquad (11)$$

If (with Laplace) we put $\cos\theta=\mu$, and for brevity

$$\frac{n^2r}{g}=m$$

and

$$\frac{\sigma}{2n}=f, \qquad\qquad\qquad\qquad (12)$$

these equations (11) become

$$a = -\frac{1}{4m}(\sin\theta)^{-\frac{s}{f}+1}\frac{\frac{d\phi}{d\mu}}{f^2-\mu^2},$$

$$b = \frac{1}{4m}(\sin\theta)^{-\frac{s}{f}}\left\{\frac{\frac{1}{f}\mu\frac{d\phi}{d\mu}}{f^2-\mu^2} - \frac{s\phi}{f^2\sin^2\theta}\right\}. \qquad \left.\right\} \quad . \quad (13)$$

Using these instead of (8) in the process by which (9) was found above, and multiplying the resulting equation by $4m\ (\sin\theta)^{\frac{s}{f}+2}$, we find

$$(1-\mu^2)^2\frac{d}{d\mu}\frac{\gamma}{f^2-\mu^2}\frac{d\phi}{d\mu} + 2\left(\frac{s}{f}-1\right)\frac{\gamma\mu(1-\mu^2)}{f^2-\mu^2}\frac{d\phi}{d\mu}$$

$$+ \left[-\frac{s^2}{f^2}\gamma + 4m(1-\mu^2)\right]\phi = -4m(1-\mu^2)\Phi. \quad (14)$$

5. To integrate this, take first the case of $\Phi=0$ (free oscillations), and assume

$$\frac{1}{f^2-\mu^2}\frac{d\phi}{d\mu} = K_0 + K_1\mu + K_2\mu^2 + \ldots + K_i\mu^i + \&c. \qquad (15)$$

This gives

$$\phi = C + f^2K_0\mu + \frac{\mu^2}{2}f^2K_1\mu^2 + \frac{\mu^3}{3}(f^2K_2-K_0) + \ldots$$

$$+ \frac{\mu^i}{i}(f^2K_{i-1}-K_{i-3}) + \&c., \quad . \quad (16)$$

where C denotes a constant of integration. Now let ϖ denote a symbol of operation such that

$$\varpi K_i = K_{i-1}, \text{ or generally } \varpi F(i) = F(i-1), \qquad . \quad (17)$$

$F(i)$ being any function of i. By aid of this notation we may write (16) short thus,

$$\phi = \Sigma\mu^i\frac{1}{i}(f^2-\varpi^2)\varpi K_i; \quad . \quad . \quad . \quad (18)$$

understanding that, when $i=0$,

$$\frac{f^2K_{i-1}-K_{i-3}}{i} = C, \quad . \quad . \quad . \quad . \quad (19)$$

and that

$$K_i=0 \text{ for all negative values of } i. \quad . \quad . \quad (20)$$

Let now $\gamma(\varpi)$ denote a symbol of operation obtained by putting ϖ for μ in γ (which, be it remembered, is a function of μ).

Then from (18) we have

$$\left[-\frac{s^2}{f^2}\gamma + 4m(1-\mu^2) \right]\phi$$

$$= \Sigma\mu^i\left[-\frac{s^2}{f^2}\gamma\langle\varpi\rangle + 4m(1-\varpi^2) \right]\frac{1}{i}(f^2-\varpi^2)\varpi\mathrm{K}_i. \quad (21)$$

Going back to (15) we have

$$\frac{\gamma\cdot\mu(1-\mu^2)}{f^2-\mu^2}\frac{d\phi}{d\mu} = \Sigma\mu^i\gamma(\varpi)\varpi(1-\varpi^2)\mathrm{K}_i, \quad (22)$$

$$\frac{d}{d\mu}\left(\frac{\gamma}{f^2-\mu^2}\frac{d\phi}{d\mu}\right) = \Sigma\mu^i\cdot(i+1)\varpi^{-1}\gamma(\varpi)\mathrm{K}_i, \quad (23)$$

and

$$(1-\mu^2)^2\frac{d}{d\mu}\left(\frac{\gamma}{f^2-\mu^2}\frac{d\phi}{d\mu}\right)$$

$$= \Sigma\mu^i(1-\varpi^2)^2(i+1)\varpi^{-1}\gamma(\varpi)\mathrm{K}_i$$

$$= \Sigma\mu^i(1-2\varpi^2+\varpi^4)(i+1)\varpi^{-1}\gamma(\varpi)\mathrm{K}_i^-$$

$$= \Sigma\mu^i[i+1-2(i-1)\varpi^2+(i-3)\varpi^4]\varpi^{-1}\gamma(\varpi)\mathrm{K}_i$$

$$= \Sigma\mu^i[i(1-\varpi^2)^2+1+2\varpi^2-3\varpi^4]\varpi^{-1}\gamma(\varpi)\mathrm{K}_i$$

$$= \Sigma\mu^i[i(1-\varpi^2)+1+3\varpi^2](1-\varpi^2)\varpi^{-1}\gamma(\varpi)\mathrm{K}_i$$

$$= \Sigma\mu^i[i(1-\varpi^2)+1+3\varpi^2](1-\varpi^2)\gamma(\varpi)\mathrm{K}_{i+1}. \quad (24)$$

Lastly, using (24), (22), and (21) in (14), and equating to zero the coefficient of μ_i, we have

$$\left\{ \left[i(1-\varpi^2)+1+\left(\frac{2s}{f}+1\right)\varpi^2\right](1-\varpi^2)\gamma(\varpi) \right.$$

$$\left. + \left[-\frac{s^2}{f^2}\gamma(\varpi)+4m(1-\varpi^2)\right]\frac{1}{i}(f^2-{}^2)\varpi^{2}\right\}\mathrm{K}_{i+1}=0. \quad (25)$$

By giving i successively in this formula all integral values from $-\infty$ to 0 and $+\infty$, and attending to (19) and (20), we have a succession of equations which successively determine K_1, K_2, K_3, &c. in terms of the arbitraries C and K_0; and using the values found in (16) we have the complete solution sought.

6. Laplace takes $\qquad \gamma = l(1-q\mu^2),$

where l and q are constants; so that the bottom and the undisturbed free surface of the water may be both elliptic spheroids of revolution. With this or any other rational integral function of μ for γ, there is no difficulty in developing (§ 7 below) the first member of (25), and working out a practical solution of the problem. Laplace's most interesting and instructive results, however, are confined to the case of an ocean of uniform depth (for which in his notation $q=0$, or $\gamma=$ constant). Taking this case first, putting

$$\frac{4m}{\gamma} = a, \quad\quad\quad\quad (26)$$

and expanding the first member of (25), we have

$$(i+1)\mathrm{K}_{i+1} + \left(-2i + \frac{2s}{f} - \frac{s^2 - \alpha f^2}{i}\right)\mathrm{K}_{i-1}$$

$$+ \left[i - \frac{2s}{f} - 1 + \frac{s^2 - \alpha f^2}{if^2} - \frac{\alpha f^2}{i-2}\right]\mathrm{K}_{i-3} + \frac{\alpha}{i-2}\mathrm{K}_{i-5} = 0. \quad (27)$$

For all negative values of i up to -2 this equation is an identity in virtue of (20) ; and for $i = -1$ it becomes

$$0\mathrm{K}_0 = 0, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (28)$$

and leaves K_0 arbitrary. For $i = 0$ it becomes, in virtue of (19) and (20),

$$\mathrm{K}_1 - \frac{s^2 - \alpha f^2}{f^2}\mathrm{C} = 0; \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (29)$$

for $i = 1$, in virtue of (20),

$$2\mathrm{K}_2 + \left(-2 + \frac{2s}{f} - s^2 + \alpha f^2\right)\mathrm{K}_0 = 0; \quad \cdot \quad (30)$$

and for $i = 2$, in virtue of (19),

$$3\mathrm{K}_3 + \left(-4 + \frac{2s}{f} - \frac{s^2 - \alpha f^2}{2}\right)\mathrm{K}_1 - \alpha\mathrm{C} = 0. \quad \cdot \quad \cdot \quad (31)$$

Then directly, for $i = 3$, $i = 4$,

$$\left.\begin{aligned}
4\mathrm{K}_4 + &\left(-6 + \frac{2s}{f} - \frac{s^2 - \alpha f^2}{3}\right)\mathrm{K}_2 \\
&+ \left(2 - \frac{2s}{f} + \frac{s^2 - \alpha f^2}{3f^2} - \alpha f^2\right)\mathrm{K} \qquad = 0, \\
5\mathrm{K}_5 + &\left(-8 + \frac{2s}{f} + \frac{s^2 - \alpha f^2}{4}\right)\mathrm{K}_3 \\
&+ \left(3 - \frac{2s}{f} + \frac{s^2 - \alpha f^2}{4f^2} - \frac{\alpha f^2}{2}\right)\mathrm{K}_1 \qquad = 0,
\end{aligned}\right\} \quad (32)$$

$$\left.\begin{aligned}
6\mathrm{K}_6 + &\left(-10 + \frac{2s}{f} - \frac{s^2 - \alpha f^2}{5}\right)\mathrm{K}_4 \\
&+ \left(4 - \frac{2s}{f} + \frac{s^2 - \alpha f^2}{5f^2} - \frac{\alpha f^2}{3}\right)\mathrm{K}_2 + \frac{\alpha}{3}\mathrm{K}_0 = 0, \\
7\mathrm{K}_7 + &\left(-12 + \frac{2s}{f} - \frac{s^2 - \alpha f^2}{6}\right)\mathrm{K}_5 \\
&+ \left(5 - \frac{2s}{f} + \frac{s^2 - \alpha f^2}{6f^2} - \frac{\alpha f^2}{4}\right)\mathrm{K}_3 + \frac{\alpha}{4}\mathrm{K}_1 = 0, \\
8\mathrm{K}_8 + &\left(-14 + \frac{2s}{f} - \frac{s^2 - \alpha f^2}{7}\right)\mathrm{K}_6 \\
&+ \left(6 - \frac{2s}{f} + \frac{s^2 - \alpha f^2}{7f^2} - \frac{\alpha f^2}{5}\right)\mathrm{K}_4 + \frac{\alpha}{5}\mathrm{K}_2 = 0,
\end{aligned}\right\} \quad (33)$$

and so on.

Of these equations, (29), (31), the second of (32), the second of (33), and every second equation thenceforward, determine successively K_1, K_3, K_5, K_7, and so forth, all in terms of the arbitrary C; and (30), the first of (32), the first and third of (33), determine successively K_2, K_4, K_6, &c. in terms of the arbitrary K_0.

7. Returning now to the more general supposition of the depth varying with the latitude, we may assume, without practically restricting the problem further,

$$\gamma = \gamma_0 + \gamma_1\mu + \gamma_2\mu^2 + \ldots + \gamma_n\mu^n, \quad . \quad . \quad (34)$$

γ_0, γ_1, $\ldots\gamma_n$ being given constants. This makes

$$\gamma(\varpi)K_i = \gamma_0 K_i + \gamma_1 K_{i-1} + \gamma_2 K_{i-2} + \ldots + \gamma_n K_{i-n}. \quad . \quad (35)$$

Using this in (25) and proceeding precisely as in § 6, we find K_1, K_2, K_3, K_4, K_5, &c., each in terms of two arbitraries C and K_0—unless γ contains only even powers of μ, in which case, as in that of uniform depth (§ 6), we find K_1, K_3, K_5, $\ldots$ in terms of one arbitrary C alone, and K_2, K_4, K_6, $\ldots$ in terms of the other arbitrary K_0 alone. The first two of the equations by which this is done, those namely which correspond to (29) and (30), being found by putting $i=0$ and $i=1$ in (25), are

$$K_1 - \left(\frac{s^2}{f^2} - \frac{4m}{\gamma_0}\right)C = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

and

$$2K_2 + 2\frac{\gamma_1}{\gamma_0}K_1 + \left(-2 + \frac{2s}{f} - s^2 + \frac{4m}{\gamma_0}f^2 + \frac{2\gamma_2}{\gamma_0}\right)K_0 - \frac{s^2}{f^2}\frac{\gamma_1}{\gamma_0}C = 0. \quad (37)$$

8. In §§ 5, 6, 7 we supposed $\Phi=0$, and so made, for the time, "free oscillations" our subject. Now suppose Φ to be any given function of μ. For the actual problem of tides of any species, it is a rational integral function of μ, or of μ and $\sqrt{(1-\mu^2)}$, if we neglect the influence produced by the change of attraction of the water due to its change of figure. A proper way of taking into account this influence by successive approximations will be explained later. Meantime, without losing generality, I assume

$$\Phi = \Phi_0 + \Phi_1\mu + \Phi_2\mu^2 + \ldots + \Phi_i\mu^i + \&c., \quad . \quad (38)$$

where Φ_0, Φ_1, Φ_2, &c. are given constants, either finite in number, or of such magnitudes as to render the series convergent for values of μ within the limits used in each particular case. With this for Φ, the second member of (14) becomes

$$-4m\Sigma\mu^i(\Phi_i - \Phi_{i-2}), \quad . \quad . \quad . \quad . \quad (39)$$

and instead of (25) we have

$$\left\{ \left[i(1-\varpi^2)+1+\left(\frac{2s}{f}+1\right)\varpi^2 \right](1-\varpi^2)\gamma(\varpi) \right.$$

$$\left. + \left[-\frac{s^2}{f^2}\gamma(\varpi)+4m(1-\varpi^2) \right]\frac{1}{i}(f^2-\varpi^2)\varpi^2 \right\} K_{i+1} = -4m(\Phi_i-\Phi_{i-2}). \quad (40)$$

The proper modification, according to this formula, must be made in (27), and in each of the particular equations (29), (30), (31), (32), (33), (36), (37) when required.

9. Before considering the conditions which may be fulfilled by proper determination of the two arbitrary constants C and K_0, it is convenient to investigate the convergency of the series (16) which we have found for the complete solution. For this purpose put (40) [including (25) as the case for which $\Phi_i=0$] into the form

$$(1-\varpi^2)^2\gamma(\varpi)K_{i+1}=\frac{1}{i}\left\{ -\left[1+\left(\frac{2s}{f}+1\right)\varpi^2\right](1-\varpi^2)\gamma(\varpi)K_{i+1} \right.$$

$$\left. + \left[\frac{s^2}{f^2}\gamma(\varpi)-4m(1-\varpi^2)\right]\frac{1}{i}(f^2K_{i-1}-K_{i-3})-4m(\Phi_i-\Phi_{i-2}) \right\}. \quad (41)$$

In a certain very important class of cases, of which the first example known to mathematicians is that so splendidly and successfully treated by Laplace in the process defended and controverted in the two preceding Numbers of this Magazine, terms of the second member of this equation are, for infinitely great values of i, comparable in magnitude with terms of the first member, through $\frac{K_{i-1}}{K_{i+1}}$ or $\frac{K_i}{K_{i+1}}$, being infinitely great of the order i^2. These cases can only occur when γ is either constant or expressed in (34) by the first two terms, $\gamma_0+\gamma_1\mu$. Reserving them for consideration later, we see by (41) that, except in those special cases, K_i must for very great values of i fulfil, more and more nearly the greater is i, the equation

$$(1-\varpi^2)^2\gamma(\varpi)K_{i+1}=0. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (42)$$

Calling κ_i the complete and rigorous solution of this equation in finite differences, we have

$$\kappa_i=l+l'i+(l''+l'''i)(-1)^i+\frac{k}{\rho^i}+\frac{k'}{\rho'^i}+\&c., \quad \cdot \quad \cdot \quad (43)$$

where ρ, ρ', &c. denote the roots of the equation $\gamma=0$, and l, l', l'', l''', k, k', &c. constants. Hence for great values of i, K_i must be approximately equal to (43) with some particular values for the constants l, l', &c. But for very great values of i all the terms of (43) except one leading term, or [because of the equal roots of $(1-\varpi^2)^2=0$] one leading pair of terms, vanish in comparison

with this term or pair of terms.   Hence we must have, for very great values of i,

$$\text{K}_i = l + l''(-1)^i, \text{ or } \text{K}_i = [l' + l'''(-1)^i]i,$$

or

$$\text{K}_i = \frac{k}{\rho^i}, \text{ or } \text{K}_i = \frac{k'}{\rho'^i}, \text{ and so on.} \qquad (44)$$

Thus we see that if each of the roots ρ, ρ', &c. is greater than unity, the series (15) and (16) are necessarily convergent for all values of μ from $\mu = -1$ to $\mu = +1$, and they are divergent for values of μ beyond these limits unless conditions proper to make $l = 0$, $l' = 0$, $l'' = 0$, $l''' = 0$ are fulfilled.   But if one or more of the roots ρ, ρ', &c. is less than unity, and ρ the absolutely least of them all, then unconditionally the series (15) and (16) are necessarily convergent for all values of μ from $-\rho$ to $+\rho$, and they are divergent for all values of μ beyond these limits unless a condition proper to make $k = 0$ is fulfilled*. When $\gamma = 0$ has imaginary roots, as $\alpha \pm \beta\sqrt{-1}$, the absolute magnitude of either of the pair is to be reckoned as $(\alpha^2 + \beta^2)^{\frac{1}{2}}$, and with this understanding the same statement as to convergency and divergency holds as for real roots.   But there is this distinction in the circumstances of the loss of convergency in the two cases, of transition through a real root and through the absolute value of a pair of imaginary roots.   In the latter case there is no discontinuity when μ is continuously increased through the critical value $\sqrt{(\alpha^2 + \beta^2)}$; in the former, ϕ and its differential coefficients become infinite and imaginary, as μ is increased continuously up to and beyond any real root of $\gamma = 0$.   The interpretation of the circumstances when imaginary roots of $\gamma = 0$ influence the solution is an exceedingly interesting subject, to which I hope to return in a future communication.   The remainder of the present article must be confined to the case of $\gamma = 0$ having two real roots, each less than unity.

10. Let ρ be any real root of $\gamma = 0$, and put $\mu = z + \rho$.   Then, for infinitely small values of z, the differential equation (14) becomes

$$a\frac{d}{dz}\left(z\frac{d\phi}{dz}\right) + bz\frac{d\phi}{dz} + (c + \mathrm{d}z)\phi = e + fz, \quad . \quad . \quad (45)$$

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* Mr. W. H. L. Russell, as I am informed by himself and Professor Cayley, has given, in perfectly general terms, this criterion for the convergency of the series in ascending powers of x for the integral of

$$\phi(x)\frac{d^2u}{dx^2} + \psi(x)\frac{du}{dx} + \chi(x) = 0,$$

in a paper communicated to the Royal Society, of which certain extracts have been published in the 'Proceedings' for 1870, 1871, 1872.

where a, b, c, d, e, f denote constants. The complete solution of this approximate equation may be found by assuming

$$\begin{aligned}
\phi = &\log z (H_0 + H_1 z + H_2 z^2 + \&c.) \\
&+ K_0 + K_1 z + K_2 z^2 + \&c.,
\end{aligned} \right\} \quad . \quad . \quad . \quad (46)$$

and determining H_1, H_2, &c. in terms of H_0, arbitrary, by equating coefficients of $\log z$, $z \log z$, $z^2 \log z$, &c. to zero, and lastly determining K_1, K_2, K_3 in terms of K_0 and H_0, each arbitrary, and H_1, H_2, H_3, &c. previously found. This shows the kind of discontinuity which any complete solution of the exact equation (14) necessarily presents when the value of μ passes through a real root of $\gamma = 0$, and how this discontinuity is averted by an assignment of the two constants of integration in the rigorous solution proper to make $H_0 = 0$ in the approximate solution (45).

11. Return now to the question (§ 9) of assigning the two constants of integration so as to fulfil any proper physical conditions of our problem. First, to work out the general solution in ascending powers of μ, use (40), and calculate K_1, K_2, &c. successively with C and K_0 arbitrary. Thus we find

$$K_i = C\alpha_i + K_0\beta_i + \Phi_0\lambda_i^{(0)} + \Phi_1\lambda_i^{(1)} + \Phi_2\lambda_i^{(2)} + \&c., \quad . \quad (47)$$

where α_i, β_i, $\lambda_i^{(0)}$, $\lambda_i^{(1)}$, $\lambda_i^{(2)}$, &c. are numbers calculated by the process, supposing f, s, m, γ_0, γ_1, γ_2, &c. to have had any particular numerical values assigned to them, and Φ_0, Φ_1, Φ_2, ..., to denote given heights. Or if before we begin the arithmetical process particular values are assigned to Φ_0, Φ_1, Φ_2, &c. so that we may put

$$\Phi_0 = n_0 L, \ \Phi_1 = n_1 L, \ \Phi_2 = n_2 L, \&c., \quad . \quad . \quad . \quad (48)$$

L denoting a given line, and n_0, n_1, n_2, &c. given numerical quantities, the result of the process of calculation of K_i from (40) will take the form

$$K_i = \alpha_i B + \beta_i K_0 + \lambda_i L, \quad . \quad . \quad . \quad . \quad (49)$$

where α_i, β_i, λ_i are calculated numbers. Then we have, by (15) and (16),

$$\left. \begin{aligned}
\frac{1}{f^2 - \mu^2} \frac{d\phi}{d\mu} &= \alpha(\mu) \cdot C + \beta(\mu) \cdot K_0 + \lambda(\mu) \cdot L, \\
\text{where} \quad \alpha(\mu) &= \alpha_0 + \alpha_1\mu + \alpha_2\mu^2 + \&c., \\
\beta(\mu) &= \beta_0 + \beta_1\mu + \beta_2\mu^2 + \&c., \\
\lambda(\mu) &= \lambda_0 + \lambda_1\mu + \lambda_2\mu^2 + \&c.,
\end{aligned} \right\} \quad . \quad . \quad (50)$$

and

$$\phi = \bar{\alpha}(\mu) \, . \, \mathbf{C} + \bar{\beta}(\mu) \, \mathbf{K_0} + \bar{\lambda}(\mu) \, . \, \mathbf{L},$$

where
$$\bar{\alpha}(\mu) = 1 + f^2 \alpha_0 \mu + \tfrac{1}{2} f^2 \alpha_1 \mu^2 + \tfrac{1}{3}(f^2 \alpha_2 - \alpha_0)\mu^3 + \&c.,$$
$$\bar{\beta}(\mu) = \quad f^2 \beta_0 \mu + \tfrac{1}{2} f^2 \beta_1 \mu^2 + \tfrac{1}{3}(f^2 \beta_2 - \beta_0)\mu^3 + \&c.,$$
$$\bar{\lambda}(\mu) = \quad f^2 \lambda_0 \mu + \tfrac{1}{2} f^2 \lambda_1 \mu^2 + \tfrac{1}{3}(f^2 \lambda_2 - \lambda_0)\mu^3 + \&c.$$
$$\tag{51}$$

It remains to determine the constants of integration so as to fulfil prescribed conditions rendering the problem determinate. This we shall actually do for two typical cases:—first, the sea bounded north and south by two vertical cliffs; secondly, by two sloping beaches with gradual deepening from each to a single maximum depth along an intermediate parallel of latitude.

12. First, let the ocean be a belt of water between vertical cliffs in two given latitudes, either both in the same hemisphere or one north and the other south. The conditions of this case are that there is no north and south motion of the water at either of the bounding parallels of latitude; and they are to be fulfilled [§ 4, (13)] by putting

$$\frac{d\phi}{d\mu} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52)$$

for each of the terminal values of μ (that is to say, the sines of the bounding latitudes). If each of these is less in absolute value than the least root of $\gamma = 0$, each of the series in (50) and (51) is convergent through the whole range of values of μ corresponding to the supposed ocean.

Calling, then, μ', μ'' the sines of the two bounding latitudes (to be reckoned negative for south latitude if either or both be south), we have, by using (50), in (52),

and
$$\begin{array}{c} \alpha(\mu') \, . \, \mathbf{C} + \beta(\mu') \, . \, \mathbf{K_0} + \lambda(\mu') \, . \, \mathbf{L} = 0, \\ \alpha(\mu'') \, . \, \mathbf{C} + \beta(\mu'') \, . \, \mathbf{K_0} + \lambda(\mu'') \, . \, \mathbf{L} = 0, \end{array} \Bigg\} \quad . \quad . \quad (53)$$

which give

$$\begin{array}{c} \mathbf{C} = - \dfrac{\beta(\mu'') \, . \, \lambda(\mu') - \beta(\mu') \, . \, \lambda(\mu'')}{\beta(\mu'') \, . \, \alpha(\mu') - \beta(\mu') \, . \, \alpha(\mu'')} \, \mathbf{L}, \\[2ex] \mathbf{K_0} = \dfrac{\alpha(\mu'') \, . \, \lambda(\mu') - \alpha(\mu') \, . \, \lambda(\mu'')}{\beta(\mu'') \, . \, \alpha(\mu') - \beta(\mu') \, . \, \alpha(\mu'')} \, \mathbf{L}. \end{array} \Bigg\} \quad . \quad . \quad (54)$$

With these values for C and $\mathbf{K_0}$, (50) and (51) give $\dfrac{1}{f^2 - \mu^2} \dfrac{d\phi}{d\mu}$, and ϕ for every value of μ through the range of the supposed ocean; and then the following formulæ [which it is convenient to recall from (13), (7), (3), (10), (38), and (48) above] give h the height of the free surface, ξ the southward displacement, and $\eta \sqrt{(1-\mu^2)}$ the eastward displacement of the water at time t, latitude $\sin^{-1} \mu$, and longitude ψ:

$$h = \left\{ \frac{\phi}{(1-\mu^2)^{\frac{ns}{\sigma}}} + \mathrm{E} \right\} \cos (\sigma t + s\psi),$$

$$\xi = -\frac{1}{4m(1-\mu^2)^{\frac{ns}{\sigma}-\frac{1}{2}}} \cdot \frac{1}{f^2 - \mu^2} \frac{d\phi}{d\mu} \cdot \cos (\sigma t + s\psi),$$

$$\eta = \frac{1}{4m(1-\mu^2)^{\frac{ns}{\sigma}}} \cdot \left\{ \frac{\mu}{f} \cdot \frac{1}{f^2 - \mu^2} \frac{d\phi}{d\mu} - \frac{s}{f^2(1-\mu^2)} \phi \right\} \sin (\sigma t + s\psi),$$

(55)

where f denotes $\dfrac{2n}{\sigma}$, and m the ratio of equatorial centrifugal force to gravity.

This fully determined solution expresses the motion of the supposed zonal ocean due to a disturbing influence, of which the equilibrium-tide height is E cos $(\sigma t + s\psi)$, E being expressed by the formula

$$\mathrm{E} = \frac{\mathrm{L}}{(1-\mu^2)^{\frac{ns}{\sigma}}} (n_0 + n_1\mu + n_2\mu^2 + \&c.), \quad . \quad . \quad (56)$$

where n_0, n_1, n_2, &c. are any given numbers.

13. If $\mathrm{L}=0$, equation (54) gives, except in a certain critical case to be considered presently, $\mathrm{C}=0$ and $\mathrm{K}_0=0$, and therefore the solution expresses determinately that there is no motion; that is to say, there cannot be any "free oscillation" of the assumed type and period, § 3, (3)[*], except in the critical case alluded to. This critical case is the case in which the denominator of the expressions for C and K_0 vanishes, or

$$\frac{\beta(\mu'')}{\alpha(\mu'')} = \frac{\beta(\mu')}{\alpha(\mu')}. \quad . \quad . \quad . \quad . \quad (57)$$

Then (54) gives infinite values to C and K_0 unless L is zero; and if L is zero, (53) gives

$$\frac{\mathrm{C}}{\mathrm{K}_0} = -\frac{\beta(\mu'')}{\alpha(\mu'')} = -\frac{\beta(\mu')}{\alpha(\mu')}, \quad . \quad . \quad (58)$$

thus determining the ratio of C to K_0 but leaving the magnitude of either indeterminate.

14. The problem of finding all the fundamental modes of free oscillation of our supposed zonal sea is solved by giving to s the values 0, 1, 2, &c., and for each value of s treating (57) as a transcendental equation for the determination of σ. After the manner of Fourier and Sturm and Liouville, it may be proved

[*] This equation defines perfectly the configuration of the assumed motion, and specifies also that its period is $\dfrac{2\pi}{\sigma}$, or its "speed" σ.

that this transcendental equation cannot have imaginary roots and has necessarily an infinite number of real roots more and more nearly equidifferent when taken in order of magnitude from the smallest positive to larger and larger positive, or from the smallest negative to larger and larger negative. In the case of $s=0$ the positive and negative roots are equal, unequal in all other cases ($s=1$, $s=2$, &c.).

15. For the convergency of the series in (50) and (51) it is necessary and sufficient (§ 9) that there be no root, real or imaginary, of $\gamma=0$ whose absolute magnitude is less than that of the absolutely greater of the two quantities μ' and μ''. But it is only when, with algebraic signs taken into account, there is a real root actually between μ' and μ'' (that is to say, when γ becomes zero for some value of μ on the direct range from μ' to μ'') that any of the six functions $\alpha(\mu)$, $\beta(\mu)$, $\lambda(\mu)$, $\bar{\alpha}(\mu)$, $\bar{\beta}(\mu)$, $\bar{\lambda}(\mu)$ used in the processes (53), (54), (57), (58), and in the final solution (51), (50), (55), is discontinuous. Why some or all of these functions should be discontinuous in this case is obvious: the sea's depth being zero along any parallel of latitude limits the physical problem to the side on which the depth is positive, or (case of equal roots of $\gamma=0$) separates the problem into two independent ones, to find the motions of the water on the two sides of a reef just "awash." An imaginary root of $\gamma=0$ having its absolute magnitude R between μ' and μ'', or a real root of contrary sign to the absolutely greater of μ' and μ'', and of absolute magnitude R between them, renders the series for $\alpha(\mu)$, $\beta(\mu)$, &c. in ascending powers of μ divergent for the portion of our range of latitude which lies beyond $\pm \sin^{-1} R$. Still the solution of the problem is fully given by (55) in terms of six functions $\alpha(\mu)$, $\beta(\mu)$, &c., *each continuous throughout the range*, but calculable, by the series in ascending powers of μ set forth in our preceding formulæ, only for the part of the range of latitude which lies between $-\sin^{-1} R$ and $+\sin^{-1} R$. The mode of dealing with the case of imaginary roots so as to obtain convenient formulæ for the numerical calculation of $\alpha(\mu)$ &c. is an interesting and important subject to which I hope to return. Being (§ 9) at present limited to the case of real roots, it is enough to remark that in this for each of the six functions $\alpha(\mu)$, $\beta(\mu)$, &c. a series continuously convergent throughout the range from μ' to μ'' may be found thus :—Let ρ and ρ' be consecutive real roots of $\gamma=0$, and let ρ, μ', μ'', ρ' be in order of algebraic magnitude. Let a be any quantity such that algebraically

and
$$\left.\begin{array}{l} a > \tfrac{1}{2}(\mu'' + \rho) \\[2mm] a < \tfrac{1}{2}(\mu\, + \rho'). \end{array}\right\} \quad \cdots \quad (59)$$

Then, putting

$$\mu = z + a \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (60)$$

in § 4 (14), and working precisely as in § 5, but with z instead of μ in the second member of (15) and the proper corresponding modification of (16) &c., we obtain a solution in ascending powers of z, or $\mu - a$, which is necessarily convergent throughout the range of our problem. The degree of convergence of the series so found for each of the six functions,

$$\alpha(z+a), \quad \beta(z+a), \quad \lambda(z+a),$$
$$\bar{a}(z+a), \quad \overline{\beta}(z+a), \quad \overline{\lambda}(z+a),$$

is, for any value of z, the same as that of the geometrical series

$$1 \pm \frac{z}{c} + \left(\frac{z}{c}\right)^2 \pm \&\text{c.},$$

where c is the less of the two quantities $a-\rho$, $\rho'-a$.

16. For our second proposed case (§ 11) let ρ_i, ρ, ρ', ρ'' be four consecutive roots of $(1-\mu^2)\gamma = 0$; let ρ, ρ' be each between -1 and $+1$; and let γ be positive for values of μ between ρ and ρ'. Required, to determine tides, and the free oscillations, of the zone of water corresponding to these intermediate values of ρ. Take any quantity, a, between ρ_i and ρ'', such that $\rho \sim a$ and $\rho' \sim a$ are each less than the less of the two differences $a-\rho_i$, $\rho''-a$. Put $\mu = z + a$, and solve in ascending powers of z, as in § 15. Let α_i, β_i, λ_i be the coefficients of z^i in the series thus found for $\alpha(\mu)$, $\beta(\mu)$, $\lambda(\mu)$ in formulæ corresponding to (50), but with z for μ in the second members, so that we have

$$\frac{1}{f^2-\mu^2}\frac{d\phi}{d\mu} = (\Sigma\alpha_i z^i)\,\mathrm{C} + (\Sigma\beta_i z^i)\,\mathrm{K}_0 + (\Sigma\lambda_i z^i)\,\mathrm{L}. \quad . \quad (61)$$

Let now p, q be two values of i, and put

$$\left.\begin{array}{c}\alpha_p\mathrm{C} + \beta_p\mathrm{K}_0 + \lambda_p\mathrm{L} = 0,\\ \alpha\mathrm{C} + \beta_q\mathrm{K}_0 + \lambda_q\mathrm{L} = 0.\end{array}\right\} \quad . \quad . \quad . \quad (62)$$

If p, q, and $p-q$* be each infinitely great, the values of C and K_0 determined by these equations and used in (61) and (55), give the tides due to the tide-generating influence

$$\Phi = \mathrm{L}(n_0 + n_1\mu + n_2\mu^2 + \&\text{c.}).$$

The periods of the fundamental free oscillations of the supposed zone of water are determined by finding σ so as to make

$$\frac{\alpha_p}{\beta_p} = \frac{\alpha_q}{\beta_q}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (63)$$

* Except in the case of $\rho-a = -(\rho'-a)$, when we must take $p-q=1$, or any odd integer; but $p-q=1$ is best in this case.

and the oscillations are then expressed by taking

$$K_0 = -\frac{\alpha_p}{\beta_p} C = -\frac{\alpha_q}{\beta_q} C \quad . \quad . \quad . \quad (64)$$

in (61) and (55). By giving moderately great values to *p*, *q*, and *p* − *q*, the rigorous solution may be satisfactorily approximated to by easy, methodical, and not very laborious arithmetic. The proof is obvious from § 9 (44).

17. Corresponding investigations to find solutions for the case of water over one or both poles must be reserved for a future article. They involve highly interesting extensions of Laplace's admirable process referred to in § 9 of the present article and in several places of the last two Numbers of the Philosophical Magazine.

Yacht ' Lalla Rookh,'
Clyde, October 14, 1875.

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XLV. *Remarks on* Mr. Croll's *" Crucial-Test " Argument.* *By* WILLIAM B. CARPENTER, *M.D., LL.D., F.R.S.*[*]

THE confidence with which Mr. Croll asserts, in his recent Papers, that the ' Challenger' Temperature-soundings furnish a " crucial test " of the Wind and Gravitation Theories of Oceanic Circulation, is not unlikely to mislead those who suppose that because Mr. Croll is an expert Arithmetician, he is to be equally trusted as an authority in Physics.

I protest, in the first place, against his putting the two doctrines in antagonism. I no more believe that the *vertical* Thermal circulation can explain all the facts of Ocean temperature, than that the *horizontal* Wind circulation is alone competent to account for them. The two circulations are not only compatible, but mutually complementary ; and as I have so treated them in all that I have written on the subject during the last five years, it is scarcely fair in Mr. Croll to ignore the fact that I have explicitly recognized the modifying influence of the Wind-circulation in the temperature of the North Atlantic, and also in that local peculiarity of the North Pacific surface-temperature to which he specially directs attention as opposed to my views. How the vast and deep basin of the North Pacific comes to be nearly filled with glacial water in any other way than by gravitation, Mr. Croll does not attempt to explain. I have clearly proved, by the example of the Mediterranean and the Red Sea, that depth *per se* has no influence on temperature, and that the deepest water in any locality will not be colder than the isocheimal of the locality unless it have flowed thither from a colder

area. That in the latitude of the Mediterranean the thermal condition of the North Pacific should be totally different, can only (as it seems to me) be accounted for by its free communication with the Antarctic area; for the Mediterranean is no more warmed by wind-currents than the North Pacific, the Gibraltar inflow (as my own observations demonstrate) being *much colder* than either the surface-water of the Atlantic outside the Strait, or the Mediterranean water within.

If he could prove, as he asserts, that the Gravitation theory makes water run up hill, this would doubtless be conclusive against it. But his asserted proof rests on an assumption which is a mere *petitio principii*—that of the equilibrium of the North Atlantic and the Equatorial columns—without which assumption no computation of their relative temperatures can possibly make out the North Atlantic column to have a higher level than the Equatorial.

But further, even if their equilibrium were admitted, there is a countervailing condition of which Mr. Croll does not seem to have taken any account, viz. the *inferior salinity* of Equatorial water, which will make a difference in the opposite direction of about 1 foot in 1026, thus neutralizing Mr. Croll's excess of $3\frac{1}{2}$ feet in a column of only 600 fathoms depth.

Again, Mr. Croll's assertion that the Gravitation theory requires a higher level for Equatorial water, rests upon the doctrine of the "gradients" necessary to keep water in motion, which theory and experience alike contradict. The researches of which Mr. Froude gave an account at the Bristol Meeting of the British Association, show that water is so nearly a "perfect fluid," that its "viscosity" may be practically disregarded when the question is one of the movement of water upon itself, not over solid surfaces. And they thus confirm the position I have all along maintained, that a slow vertical circulation will be kept up through the whole length of a basin filled with water that is continuously subjected to surface-cold at one end and to surface-heat at the other, without any *appreciable* difference of level between the two extremities. As an instance of the slight difference in sp. gr. which will suffice to maintain such a movement, Mr. Froude informed me that in an extensive examination of harbours, lochs, fiords, &c. he had found that a very small dilution of their contents by fresh water is sufficient to produce an inward underflow of sea-water, tending to restore the disturbed equilibrium.

The doctrine of a vertical Thermal circulation was first put forward by Lenz (well known to his contemporaries as a most able physicist) nearly thirty years ago, as the only one which would account for the phenomena of Deep-sea temperature ascertained

by him in Kotzebue's second voyage (1823–26). It was accepted by Pouillet and Arago ; and, though put aside for the doctrine of a uniform Deep-sea temperature of 39°, supposed to have been established by the observations of D'Urville and Sir James Ross, it was finally accepted explicitly by Sir John Herschel. Sir G. Airy, in his Presidential Address for 1872, spoke of it as "certain in theory and supported by observation." And Sir William Thomson, who has kindly allowed me on various occasions to appeal to his profound mastery of the subject as a test of the correctness of my own conclusions, most emphatically stated in the Geographical Section at Bristol, after hearing Mr. Croll's Paper and my reply to it, that the doctrine " is not a matter of argument, but of irrefragable demonstration."

On these grounds, therefore, I venture to ask for an " arrest of judgment" in regard to the value of Mr. Croll's "crucial test." Until indorsed by some competent physical authority, I submit that it rests on no better basis than his own inadmissible assumption, which, as can be easily shown (see ' Nature,' Oct. 21), would make the demonstrated inward underflow of Ægean water in the Dardanelles as complete an impossibility as he asserts the Thermal Oceanic circulation to be.

It is satisfactory, however, to learn from Mr. Croll's last communication to ' Nature ' (Oct. 7, 1875), that he now allows physicists to have an opinion of their own in regard to the sufficiency of differences of Oceanic temperature to produce circulation. And I am not without hope, therefore, that he may come in time to admit physicists to be in the right on a question which they alone are competent to decide. If I had not found my own views to be indorsed by their assent, I should have at once abandoned them.

XLVI. *On* Kohlrausch's *Determination of the Absolute Value of the Siemens Mercury Unit of Electrical Resistance.* By Professor A. Stoletow.

To the Editors of the Philosophical Magazine and Journal.

University of Moscow,
October 1 (13), 1875.

Gentlemen,

IN a Note recently published in your Magazine (September 1875), Mr. H. A. Rowland points out an error in Kohlrausch's determination of the Siemens unit of resistance, and expects that it will account for the difference between Kohlrausch's result and that of the Committee on Electrical Standards.

The correction, arising from self-induction of the circuit in Kohlrausch's experiments, does indeed tend to reconcile the two.

results; but it is far less considerable than Mr. Rowland seems to expect. More than a year ago, while reading (in Maxwell's 'Electricity,' vol. ii. p. 361) the exact theory of Weber's method, adopted by Kohlrausch, I calculated this correction. The data for the earth-inductor I take from a paper of Weber's (*Ueber die Anwendung d. magn. Induction auf Messung d. Inclination*, Göttingen, 1853, p. 53); and those for the galvanometer-coil may be calculated approximately from Kohlrausch's description and sketch. I assume the windings of the galvanometer to be of a rectangular form, the innermost being 180 millims. long by 70 wide, the outermost 260 by 150, and the number of windings $= 250$ (it was "about 250" according to Kohlrausch). Calculating the coefficients of self-induction L by Maxwell's method ('Electricity,' vol. ii. chap. 13), I find $L_1 = 9 \cdot 170 \cdot 10^8$ millims. for the inductor, $L_2 = 0 \cdot 822 \cdot 10^8$ for the galvanometer; hence the value of L for the whole circuit $L = L_1 + L_2 = 9 \cdot 992 \cdot 10^8$ millims. (the distance between the two parts of the system being sufficiently great to make the coefficient of mutual induction insensible). From this value of L I calculate the period of vibration T of Kohlrausch's magnet (with the circuit closed) by Maxwell's equation (15) (*l. c.* p. 363), the other coefficient $\dfrac{\mathrm{G}^2 m^2}{\mathrm{A}^2}$ in the equation being estimated from Kohlrausch's data with an accuracy quite sufficient for our purpose. The said equation I solve by successive approximations, taking first the right side $= 0$ (Kohlrausch's assumption) and calculating T, then putting this value of T in the right side to obtain the second approximation, and so forth. I find $T = 34 \cdot 8636$ sec., and proceed now to calculate the exact value of the resistance R of the circuit, using Maxwell's equation (14). This gives, if we denote by R_K the value of R as found by Kohlrausch,

$$R = R_K (1 + \delta) - \Delta,$$

where

$$\delta = 0 \cdot 00035, \quad \Delta = 0 \cdot 00281 \cdot 10^{10} \frac{\text{millim.}}{\text{sec.}}$$

Finally, from the three measurements made by Kohlrausch I find

$$1 \text{ Siem.} = 0 \cdot 97000 \cdot 10^{10} \frac{\text{millim.}}{\text{sec.}},$$

$$\text{,,} \quad 0 \cdot 97174 \quad \text{,,} \quad \text{,,}$$

$$\text{,,} \quad 0 \cdot 97242 \quad \text{,,} \quad \text{,,}$$

and the mean result will be

$$1 \text{ Siem.} = 0 \cdot 97139 \cdot 10^{10} \frac{\text{millim.}}{\text{sec.}},$$

instead of $0 \cdot 9717 \cdot 10^{10}$ as given by Kohlrausch.

A detailed account of this calculation is about to be published in the 'Journal of the Physical Society of St. Petersburg.'

I am, Gentlemen,

Yours most respectfully,

A. STOLETOW,

Professor of Physics.

XLVII. *Notices respecting New Books.*

Geological Survey of England and Wales. Guide to the Geology of London and the Neighbourhood. By WILLIAM WHITAKER, *B.A., F.G.S.* 8vo, pp. 72. London: Longmans and Co. and E. Stanford. 1875.

THIS is an explanation of the Geological-Survey Map of London and its environs, and of the Geological Model of London in the Museum of Practical Geology in Jermyn Street, London.   The map was one of the special geological maps, instituted by the late Director-General Sir R. I. Murchison, showing the superficial gravels, sands, loams, peat, &c., which are usually ignored in common geological maps, but are really of great importance to the agriculturist, horticulturist, and house-occupier, and to the constructors of houses and other buildings, docks, canals, sewers, and surface-drains. The model—one of the finest and most accurate ever made—has a horizontal scale of 6 inches to the mile; and, having a length of about 15 miles E. and W. and a breadth of about 11 miles N. and S., represents an area in and around London of about 165 square miles. This is divided into nine four-sided masses of unequal size, bounded by lines known to afford sections from wells and borings.   Five of these portions can be raised by machinery, worked by a handle, so as to bring up and show the sections along the inner sides of each.   The vertical scale was necessarily exaggerated, and is about 4·4 times as large as the horizontal; but this does not appear excessive. This model was successfully made under the superintendence of Messrs. Whitaker, Jordan, and Brion, and placed in the Museum in 1873. A description of the model and of its construction, and of the several lines of section traversing it, is given in full at pages 4–13.

The body of this valuable memoir is occupied with the description of the several geological formations known to constitute the soil, subsoil, substrata, and foundations of the London area. This great series of earthy and stony materials is briefly, but pointedly, described from the lowest known strata upwards; and very much information is given as to the water-supply and the local features and scenery, dependent on the structure (pages 14–68).   Detailed descriptions of the deposits, sections, and fossils have been already published in the larger and still richer memoir on the whole of the London District, by Mr. Whitaker and his colleagues, in vol. iv. of the 'Memoirs of the Geological Survey,' 1872.

We may here enumerate the strata downwards, from the surface, with their greatest thickness in this area, as :—

I. The Alluvium, or Recent River-deposits, about 15 feet thick.

II. Post-pliocene Tertiaries. 1. *Old River-drift, Post-glacial* : *a.* Brickearth or Loam, 30 feet ; *b.* Gravel and sand, 20 feet ; *c.* Plateau Gravel of doubtful age, about 20 feet. 2. *Surface-deposits on the Chalk-tract* : *a.* Brickearth and pebbly loam, about 30 feet ; *b.* Clay with flints, of doubtful age ; thin. 3. *Glacial Drift* : *a.* Boulder-clay (" Upper Glacial "), about 40 feet ; *b.* Gravel, sand, and clay, interbedded (" Middle Glacial "), about 40 feet. 4. Pre-glacial (?) Pebble-gravel, about 20 feet.

III. Eocene Tertiaries. 1. *Bagshot Beds* : *a.* Upper Bagshot Sand, about 20 feet ; *b.* Bracklesham or Middle Bagshot Beds, about 10 feet ; *c.* Pebble-beds, about 30 feet, and Sand and loam, about 150 feet, together representing the Lower Bagshot Beds. 2. *Lower London Tertiaries* : *a.* The London Clay, 450 feet ; *b.* Old-haven or Blackheath Beds, 50 feet ; *c.* Woolwich and Reading Beds, 90 feet ; *d.* Thanet Sand, 50 feet.

IV. Cretaceous. 1. *Chalk*, 800 feet ; 2. *Upper Greensand*, about 30 feet ; 3. *Gault*, about 160 feet.

The most noteworthy exposed sections of the different beds and formations are carefully tabulated for each group, showing the nature of the beds seen there, and their relative place in the series to which they belong.

Of the lowermost strata found by the deep boring (1302 feet) at Kentish Town, in 1853, the lowest 188 feet consisted of clays, sands, sandstones, and conglomerates, mostly reddish, which have puzzled geologists, but at all events indicate a deep-seated range of older rocks covered by the Lower Cretaceous deposits in a different condition and of different structure from that of the Lower Greensand on the north and south of the London area. At Harwich a boring, 1070 feet deep, also penetrated the Cretaceous rocks, and pierced $44\frac{1}{2}$ feet into Lower Carboniferous (Posidonomya) rock. In relation to these facts, Mr. Whitaker truly observes at p. 16 :—

" One of the most striking pieces of theoretical geology, or of philosophical inference from observed facts, is Mr. Godwin-Austen's argument that the two exposures of the older rocks in the Ardennes (Belgium) and in our own Mendip Hills (Somerset) are parts of one great axis or line of elevation, and that they are most likely connected underground by a hidden range of those older rocks nearly along the valley of the Thames and the Wealden eleva-tion—an important conclusion that was to a great extent verified by the sinking of the Harwich well, and also by the doubtful nature of the bottom beds in the Kentish-Town well. Although the fur-ther argument, that veritable Coal-measures may occur along the lines indicated has not yet passed out of the domain of inference, and though, should beds occur, we cannot be certain they would contain workable coal, yet geologists are fully justified in pointing to the whole chain of reasoning as one that shows how pure science

may have a direct bearing on questions of the highest importance from a most utilitarian point of view."

The frontispiece is a woodcut-diagram showing the probable position of these lowest and far more ancient rocks, crumpled up and worn down, on which the Lower Cretaceous sands and clays lie unconformably.

Many points of interest to the geologist, whether tyro or advanced, occur throughout this memoir. The mistaken notion that half-taught students get of the so-called London "Basin" is held up to caution (p. 3), and the relation of this synclinal trough to the saddle-back of the Weald, and the hollow fold of the Hampshire area is noticed (p. 69), as well as the subordinate flexures and faults at Windsor, Deptford, and Purfleet (pp. 21 and 70), like that at Portsdown (we may add), though smaller, and others, which may affect the well-being of the proposed Channel Tunnel. The subterranean erosion of the Chalk by acidulated waters is noticed at pp. 25, 26. The nature of flint is referred to at p. 19 ; but nothing satisfactory or new is advanced ; and the very probable theory of pseudomorphism of silica after limestone is not noticed, though applicable even to the so-called vein-flint, where on both sides of a fissure the chalk itself, with some of its fossils, is changed into flint, as in the nodules themselves.

This admirably concise and useful memoir ends with a well-written page or two on " Denudation, its nature and effects." But we must be allowed to doubt whether the denudation, or surface-sculpture, of the London district has been wholly worked out by rain and rivers. It was not so very long ago, geologically speaking, that a wide expanse of shallow waters (whether marine, estuarine, or freshwater, is not yet clear) left banks and shoals and wide areas of gravel, sand, and loam spread out over all the district from Chalk-range to Chalk-range (referred to at p. 57). These shallow waters must have retreated, slowly or rapidly, uniformly or by fits and starts ; and both river-currents and retreating tides leave valley-furrows on deserted shoals and banks of mud and shingle. Hence it would be strange indeed if the gravels, clays, and sands of this district, so near the present level of the sea and connected with British ground that has subsided even 1000 feet below the Glacial Sea and risen again, should not have been worn by the retreating sea into valleys of drainage which have since been widened out and eaten further back by snow and ice and rain and rivers.

For all who wish to understand the geology of London and its neighbourhood, the map, the model, and the memoir are indispensable ; and an intelligent interest, indeed a philosophic life, is imparted to the dry details of descriptive geology by the comprehensive and well directed knowledge of the talented author of the memoir.

XLVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 331.]

December 2, 1874.—John Evans, Esq., F.R.S., President,
in the Chair.

THE following communications were read:—

1. " On the Femur of *Cryptosaurus eumerus* (Seeley), a Dinosaur
from the Oxford Clay of Great Gransden." By Harry Govier
Seeley, Esq., F.L.S., F.G.S., Professor of Physical Geography in the
Bedford College, London.

The author described this femur as showing a slight forward bend
in the lower third of the shaft, and as having the terminal portions
wider in proportion to the length of the bone than in any described
Dinosaurian genus. He pointed out its differences from the cor-
responding bone in *Megalosaurus*, *Iguanodon*, and other genera.
The length of the femur was stated to be about 1 foot.

2. " On the Succession of the Ancient Rocks in the vicinity of
St. David's, Pembrokeshire, with special reference to those of the
Arenig and Llandeilo groups and their fossil contents." By Henry
Hicks, F.G.S.

In the first part of this paper the author described the general
succession of the rocks in the neighbourhood of St. David's from the
base of the Cambrian to the top of the Tremadoc group, and showed
that they there form an unbroken series. The only break or un-
conformity recognized is at the base of the Cambrian series, where
rocks of that age rest on the edges of beds belonging to a præ-
Cambrian ridge.

In the second part the author gave a minute description of the
rocks, comparing the Arenig and Llandeilo groups, as seen in Pem-
brokeshire with each other, and also with those known in other
Welsh areas.

Each group he divided into three subgroups, chiefly by the fossil
zones found in them.

1. The *Lower Arenig* was stated to consist of a series of black
   slates about 1000 feet thick, and to be characterized chiefly
   by a great abundance of dendroid graptolites.

2. *Middle Arenig*. A series of flags and slates, about 1500 feet
   thick, and with the following fossils—*Ogygia scutatrix, O. pel-
   tata, Ampyx Salteri*, &c.

3. *Upper Arenig*. A series of slates, about 1500 feet in thick-
   ness, only recently worked out, and found to contain a large
   number of new and very interesting fossils belonging to the
   following genera—viz. *Illænus, Illænopsis, Placoparia, Bar-
   randia*, &c.

4. *Lower Llandeilo*. A series of slates and interbedded ash,
   equivalent to the lowest beds in the Llandeilo and Builth dis-

tricts, and containing species of *Æglina, Ogygia, Trinucleus,* and the well-known graptolites *Didymograptus Murchisoni* and *Diplograptus foliaceus,* &c.

5. *Middle Llandeilo.* Calcareous slates and flags with the fossils *Asaphus tyrannus, Trinucleus Lloydii, Calymene cambrensis,* &c.
6. *Upper Llandeilo.* Black slates and flags, with the fossils *Ogygia Buchii, Trinucleus fimbriatus,* &c.

The Arenig series was first recognized in North Wales by Prof. Sedgwick about the year 1843, and was then discussed by him in papers presented to the Society. The Llandeilo series was discovered by Sir R. Murchison previously in the Llandeilo district ; but its position in the succession was not made out until about 1844. The Geological Survey have invariably included the Arenig in the Llandeilo group ; but it was now shown that this occurred entirely from a mistaken idea as to the relative position of the two series, which were now shown to be entirely distinct groups, the equivalents of both groups being present in Carnarvonshire, Shropshire, and Pembrokeshire, but the Llandeilo group only of the two being developed in Carmarthenshire.

The lines of division in the series were said to be strongest at the top of the Menevian group and at the top of the Tremadoc group, these lines being palæontological breaks only, and not the result of unconformities in the strata.

December 16, 1874.—John Evans, Esq., F.R.S., President,
in the Chair.

The following communications were read :—

1. " Descriptions of the Graptolites of the Arenig and Llandeilo Rocks of St. David's." By John Hopkinson, Esq., F.G.S., and Charles Lapworth, Esq., F.G.S.

Commencing with a brief historical account of the discovery of Graptolites in the neighbourhood of St. David's, from their first discovery in the Llandeilo series in 1841 by Sir Henry De la Beche and Professor Ramsay, the authors proceeded to explain their views on the classification of the Graptolites (GRAPTOLITHINA, Bronn), which they place under the order *Hydroida,* dividing them into two groups :—RHABDOPHORA (Allman), comprising the true siculate or virgulate Graptolites, which they consider to have been free organisms ; and CLADOPHORA (Hopkinson), comprising the dendroid Graptolites and their allies, which were almost certainly fixed, and are most nearly allied to the recent *Thecaphora.*

The distribution of the genera and species in the Arenig and Llandeilo rocks of St. David's was then treated of ; and the different assemblages of species in each of their subdivisions were compared with those of other areas.

The Arenig rocks are seen to contain a number of species which ally them more closely to the Quebec group of Canada than to any other series of rocks, all their subdivisions containing Quebec species ; while the Skiddaw Slates, which before the discovery of Graptolites in the Lower Arenig rocks of Ramsey Island in 1872

were considered to be our oldest Graptolite-bearing rocks, can only be correlated with the Middle and Upper Arenigs of St. David's. The Graptolites of the Arenig rocks of Shropshire and of more distant localities were also compared with those of St. David's.

In the Llandeilo series of this district the Cladophora have now for the first time been found,—a few species, with several species of Rhabdophora, occurring at Abereiddy Bay in the Lower Llandeilo, which alone has been carefully worked—there being much more to be done in the Middle and Upper Llandeilo, from which very few species of Graptolites have as yet been obtained.

Some of the recently introduced terms, and altered or more definite terminology, employed in the descriptions of the species were then explained; and the paper concluded with descriptions of all the species of Graptolites, collected in the Arenig and Llandeilo rocks of St. David's within the last few years, of which sufficiently perfect specimens have been obtained, doubtful species being referred to in an appendix.

Forty-two species were described, belonging to the following genera: —*Didymograptus*, *Tetragraptus*, *Clemagraptus* (gen. nov.), *Dicellograptus*, *Climacograptus*, *Diplograptus*, *Phyllograptus*, *Glossograptus*, and *Trigonograptus* (Rhabdophora); *Ptilograptus*, *Dendrograptus*, *Callograptus*, and *Dictyograptus* (Cladophora).

2. " On the Age and Correlations of the Plant-bearing series of India, and the former existence of an Indo-Oceanic Continent." By H. F. Blanford, Esq., F.G.S.

In this paper the author showed that the plant-bearing series of India ranges from early Permian to the latest Jurassic times, indicating that, with few and local exceptions, land and freshwater conditions had prevailed uninterruptedly over its area during this long lapse of time, and perhaps even from an earlier period. In the early Permian there is evidence, in the shape of boulder-beds and breccias underlying the lowest beds of the Talchir group, of a prevalence of cold climate down to low latitudes in India, and, as the observations of geologists in South Africa and Australia would seem to show, in both hemispheres simultaneously. With the decrease of cold the author believed the flora and Reptilian fauna of Permian times were diffused to Africa, India, and perhaps Australia; or the flora may have existed somewhat earlier in Australia, and have been diffused thence. The evidence he thought showed that during the Permian epoch India, South Africa, and Australia were connected by an Indo-oceanic continent, and that the first two remained so connected, with at the utmost some short intervals, up to the end of the Miocene period. During the latter part of the time this continent was also connected with Malayana. The position of the connecting land was said to be indicated by the range of coral reefs and banks that now exists between the Arabian Sea and West Africa. Up to the end of the Nummulitic epoch, except perhaps for short periods, no direct connexion existed between India and Western Asia.

January 13, 1875.—John Evans, Esq., F.R.S., President,
in the Chair.

The following communications were read:—

1. " On the Kimmeridge Clay of England." By the Rev. J. F.
Blake, M.A., F.G.S.

The author described in considerable detail the development of the
Kimmeridge Clay in various parts of England, dwelling especially
upon the palæontological phenomena presented by it in the different
localities. He arrived at the conclusion that the Kimmeridge Clay
in England is divisible only into two sections, Upper and Lower;
but when it is preceded by the Coral Rag, it possesses a basal series
of no great thickness, which may be designated the Kimmeridge
Passage-beds. He compared his Upper Kimmeridge with the lower
part of the " Virgulien " of foreign authors. It consists of paper
shales, paper slabs, bituminous shales, and cement stones, with
interstratified clays, and may attain a thickness of at least 650 feet.
Its fauna is characterized by paucity of species and great abundance
of individuals. It is thickest in Dorsetshire and Lincolnshire, but
thin or absent in the inland counties. The author stated that no Fauna
comparable with that of the Middle Kimmeridge or " Ptérocérien "
has been discovered in England, though some of its less-charac-
teristic fossils occur associated with Lower-Kimmeridge forms. The
Lower Kimmeridge is a mass of blue or sandy clay, with numerous
calcareous " doggers," largely developed in Lincolnshire, the whole
representing the " Astartien " of foreign geologists. Its thickness is
estimated at from 300 to 500 feet in Ringstead Bay, and about 400
feet in Lincolnshire. The fossils of the Coral Rag extend up into
the Kimmeridge passage-beds, which are typically developed at Wey-
mouth, where they are about 20 feet thick.

2. " Note on *Pelobatochelys Blakei* and other Vertebrate Fossils
obtained by the Rev. J. F. Blake from the Kimmeridge Clay." By
Harry Govier Seeley, Esq., F.L.S., F.G.S., Professor of Physical
Geography in the Bedford College, London.

The author stated the fossils referred to in his paper gave evidence
of three species of *Ichthyosaurus* (one larger than any previously
known to occur in the formation), a *Pliosaurus*, a *Stereosaurus*, a
small Ornithosaurian, and a species of Chelonian, which he described
under the name of *Pelobatochelys Blakei*. The remains of this
animal indicated a carapace 16 inches long by 14 inches broad, and
angularly arched posteriorly. The pygal scute was divided as in
Emys; and the hinder margins of the vertebral scutes were elevated
as in some species of *Batagur*. The vertical scutes were nearly
twice as broad as long, and interlaced with each other by sawlike
margins. The costal plates were imperfectly ossified.

3. " On the Cambridge Gault and Greensand." By A. J. Jukes-
Browne, Esq., F.G.S.

This paper has for its object to determine the true position of the
Cambrige nodule-bed in the Cretaceous series, and to investigate the
nature and origin of its peculiar fauna.

The first part of the paper deals with the stratigraphical relations of the beds; and the author calls attention to the fact that in the numerous artificial sections near Cambridge only two formations are really visible, viz. the Chalk Marl with a pebble-bed of phosphatic nodules at the base, and the stiff dark clay of the Gault, upon which these rest.

The so-called Greensand or nodule-bed passes up into the Chalk Marl, but rests unconformably on the Gault below, which presents in fact a *surface of erosion*; and there is therefore a *break of indefinite length* between the Cambridge Gault and Greensand.

The nodule-bed continues to present much the same characters and fossils through Bedfordshire as far as Sharpenhoe, a village about 3 miles east of Harlington, on the Midland Railway. Here is situated the most westerly coprolite pit or working in the Cambridge beds; and beyond this the Gault passes into Chalk Marl without any such seam intervening.

It is not until we enter Buckinghamshire and reach Buckland near Tring, that any thing like true Upper Greensand appears, and separates the Chalk Marl from the Gault. From this point westward the formation increases in thickness and importance; but its characters and fossils are quite different from those of the Cambridge Greensand.

Although in Bucks no coprolites are found between the Gault and Greensand, yet they occur in the Gault itself; and one bed may be traced towards the N.E., and is found to commence where the Cambridge nodule-bed ends, thereby raising the presumption that it becomes confluent with that bed, and has furnished many of the well-known fossils and nodules it contains.

A consideration of these facts warrants the following general conclusions :—

    I. That the Cambridge Greensand or nodule-bed has no connexion with the Upper Greensand, its actual position being at the base of the true Chalk Marl.

    II. That the same bed rests unconformably on the clay below, and that its coprolites and fossils have been derived from the Gault.

    III. That in consequence of this erosion a great gap now exists in Cambridgeshire between the Lower Gault and the Chalk Marl, the whole of the Upper Gault and Upper Greensand being absent.

The palæontological evidence leads to exactly the same conclusions. The fauna is divisible into 2 groups; and the fossils belonging to the one are preserved in dark phosphate, and being generally waterworn are clearly derived forms, while the others are of lighter colour and belong to the deposit. The former group is chiefly composed of Gault species, 70 per cent. of which belong to the upper stage of that formation; while the fossils proper to the deposit are also found in the Chalk Marl above.

The author therefore feels justified in concluding that stratigraphically the bed is Chalk Marl, while palæontologically considered its fauna is mainly derived from the Upper Gault.

XLIX. *Intelligence and Miscellaneous Articles.*

ARTIFICIALLY CRYSTALLIZED OXIDE OF ZINC FROM A BLAST-FURNACE. BY RICHARD COWPER, ASSOCIATE OF THE ROYAL SCHOOL OF MINES.

ON the occasion of the removal of the lining of one of the blast-furnaces at the Tredegar Iron Works, a crystalline substance was observed coating the bricks of the hearth and boshes, by Mr. James Colquhoun, the manager of the works. On examining the bricks, I obtained some bright and transparent but rather small crystals in sufficient quantity for examination. They proved to be practically pure oxide of zinc, which, I am informed by Dr. Percy, is well known as a crystallized blast-furnace product.

An analysis gave 100·13 per cent. ZnO, with a trace of Al_2O_3 and Fe_2O_3, possibly due to adherent particles of the brick which formed the " gangue."

The crystals are yellow and greenish yellow in colour, transparent to translucent, and belong to the hexagonal system, the following forms being noticed :—

$$(1)\quad \infty P . OP. \qquad (2)\quad \infty P . P.$$
$$(3)\quad \infty P . OP . P. \qquad (4)\quad \infty P . \infty P2 . OP . P.$$

The native oxide of zinc, zincite or spartalite, also belongs to the hexagonal system ; but crystals of this mineral appear to be rare.

Tredegar Iron Works.

ON THE CHEMICAL AND SPECTROSCOPIC CHARACTERS OF A NEW METAL (GALLIUM). BY LECOQ DE BOISBAUDRAN.

The day before yesterday (August 27, 1875) I found some indications of the probable existence of a new element, in the products of the chemical examination of a blende from the Pierrefitte Mine, Valley of Argelès, Pyrenees.

I have at present ascertained as follows :—

(1) The oxide (or, perhaps, a subsalt) is precipitated after some time by metallic zinc in a solution containing chlorides and sulphates. It does not appear to be the metal itself reduced by the zinc.

(2) The chloride is precipitated by a small quantity of ammonia. In a mixture containing excess of chloride of zinc the new substance is precipitated before the zinc when the liquid is treated with an insufficient quantity of ammonia to throw down the latter. After the second precipitate the proportion becomes minute, almost the whole being found in the first fraction.

(3) Even in conditions which must correspond to a state of peroxidation[*], the oxide is soluble in excess of ammonia.

[*] On the hypothesis that the substance has two oxides, analogous to those of iron.

(4) The salts are precipitated by the sulphydrate of ammonia, excess of which does not seem to redissolve perceptibly the sulphide formed.

(5) The salts are precipitated by sulphydric acid in presence of acetate of ammonia and much free acetic acid. In the presence of zinc the new body is concentrated in the first-deposited sulphides; it nevertheless required six successive precipitations to effect the almost complete disappearance of the sulphide of zinc.

(6) Sulphydric acid in solution slightly acidulated by chlorhydric acid does not precipitate the salts.

(7) The oxide redissolves in excess of carbonate of ammonia, together with the zinc.

(8) The extremely small quantity of substance at my disposal has not permitted the isolation of the new body from the accompanying excess of zinc. The few drops of chloride of zinc in which I have *concentrated* the new substance give, under the action of the electric spark*, a spectrum composed principally of a violet line, narrow, readily visible, situated nearly at 417 on the scale of wave-lengths. I also perceived a very faint line towards 404.

I am pursuing this investigation, and hope in a few days to procure a little more of the old material, in order to determine the reactions of the new substance.

[The following addition to the above was presented at the sitting of the Academy, Sept. 29.]

The experiments which I have made since August 29 confirm me in the notion that the substance noticed must be considered a new element, to which I propose to give the name of *Gallium*.

(9) The sulphide is really insoluble in excess of sulphydrate of ammonia.

(10) Although the quantity at my disposal is still very small, I have obtained the chloride in such a state of concentration that the line 417 is sufficiently bright under the action of the induction-spark.

(11) The chloride gives the line 417 in the gas-flame, but more feeble than with the electric spark exploding upon the solution.

(12) The salts are readily precipitated, without heating, by carbonate of barium.

(13) In a mixture containing a large excess of chloride of zinc the new substance is thrown down by sulphydrate of ammonia, with the first portions of the sulphide of zinc.

(14) Repeated evaporations with much excess of aqua regia do not appear to occasion any loss of chloride by volatilization.

(15) It seems to me that the sulphide will be white, like that of zinc. This point has to be cleared up after complete purification of the substance.

(16) When hydrated chloride of zinc containing traces of the new body is heated up to the point at which a small quantity of

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* Employing the small tubes described in my work *Spectres Lumineux*, p. 15.

oxychloride of zinc is formed, all the gallium remains insoluble [*] —under the form of oxychloride, I suppose.

(17) The spectrum is brighter with a spark of average length than with a very short one.—*Comptes Rendus de l'Académie des Sciences,* Sept. 20, 1875, pp. 493–495.

ON THE INFLUENCE OF LIGHT UPON THE CONDUCTIVITY OF CRYSTALLINE SELENIUM. BY WERNER SIEMENS.

The property of crystalline selenium first described by Willoughby Smith, and more closely studied by Sale[†], viz. that in the illuminated condition it is a better conductor of electricity than in darkness, I have further investigated, and established the correctness of the facts. The specific conductivity, however, of selenium made crystalline by heating to 100° or 150° C. is very slight and extraordinarily variable; the augmentation of the conductivity by illumination is also very inconstant; so that it was impossible to demonstrate a definite dependence of the conductivity upon the illumination. But by protracted heating of amorphous selenium up to the temperature of 210° C., and also by cooling melted selenium to the same temperature of 210° C. (at which, with longer duration of it, the selenium passes into a coarsely granular crystalline state), I succeeded in producing another modification of crystalline selenium, which possesses and permanently retains a considerably greater conductivity, and conducts electricity after the manner of metals, its conducting-power diminishing with rise of temperature. Also the action of light upon it is far greater, and appears to be perfectly constant. By fusing into coarsely granular selenium two flat spirals of wire, at the distance of about 1 millim. from each other, between two thin plates of mica, I produced an extraordinarily sensitive photometer. With it obscure heat-rays are *without direct influence* upon the conductivity, which is *diminished* by heating the selenium; while diffused daylight doubles it, and direct sunlight in some circumstances raises it to *more than tenfold.* The augmentation of the conductivity of the coarsely granular selenium by illumination proceeds with extraordinary rapidity. Likewise the lessening of it on shutting off the light appears to commence instantaneously; yet a longer time elapses before the state corresponding to darkness is completely restored. The increment of the conductivity is not proportional to the light-intensity, but a function of it which comes nearer to the ratio of the square root of that intensity.

I defer communicating details on this interesting property until the conclusion of my experiments, merely remarking that I hope to be able to utilize them for the construction of a reliable photometer.—*Monatsbericht der kön. preuss. Akademie der Wissenschaften zu Berlin,* 1875, p. 280.

[*] But readily soluble in a few drops of chlorhydric acid.
[†] Proc. Royal Society, vol. xxi. p. 283; Pogg. *Ann.* vol. cl. p. 333.

L. *Spectral-Analytical Researches.*
By R. Bunsen[*].
[With a Plate.]

THE low temperature of the non-luminous gas-flame suffices for the production of but a few of the spectra of the simple substances and of their compounds; by far the greater number of the elements are vaporized at temperatures which can only be attained by means of the electric spark. The employment of spark-spectra in analytical research cannot be dispensed with in those cases where a new element is sought for, or where indubitable proof is required of the presence of substances possessed of properties so similar as to render uncertain their detection and separation by ordinary reagents. So many difficulties, however, stand in the way of the employment of spark-spectra, that this important method of analysis has not as yet obtained a practical recognition in chemical laboratories. Means have been wanting for the production at any time of spark-spectra with the same ease as attends the formation of flame-spectra; the first part of this paper will therefore be occupied with the description of a battery and spark-apparatus which supply this want. The non-existence of Tables of spectra suited to all practical requirements is another difficulty in the way of the employment of these spectra.

There are, it is true, a number of measurements, for the most part excellent; but the purity of the material employed in

the experiments upon which the measurements are based is doubtful. On attempting to reduce to a common scale the spectra obtained by different observers working with different dispersive powers, different breadths of slit, now at high, now at lower temperatures, Tables are obtained which are altogether useless for laboratory purposes. In order to obtain a sure standing-ground, preliminary researches into the purity of the material employed for fundamental observations are necessary, inasmuch as doubts about individual lines can only be set at rest when it is known that the material employed is perfectly pure. Graphic representations of spectra are valueless for practical work, if every uncertainty with regard at least to the principal lines is not removed. The methods employed in the following research for the production of perfectly pure materials are, where it appeared necessary, discussed in detail.

I. Battery and Spark-apparatus.

No one who is in the habit of using currents of great intensity with temporary interruptions of days, weeks, or months, but must have experienced much inconvenience from setting up, taking to pieces, and cleaning the constant batteries which are ordinarily employed. So long as the production of spark-spectra continues to involve such tedious and troublesome operations, it is not to be expected that these spectra will be generally used in chemical laboratories; the battery about to be described (which is without clay cells) is designed to do away with these difficulties.

I have already shown that a mixture of potassium dichromate and sulphuric acid may advantageously replace the nitric acid of the carbon-zinc battery without clay cells. More recently Leeson and Warrington have proposed to use this mixture in a battery with clay cells, in such proportions as shall ensure the production of chrome alum (by the action of the sulphuric acid upon the dichromate) and the solution of this salt in the water present. Such a liquid is produced by mixing together—

Potassium dichromate . . . . 1·33 part.
Concentrated sulphuric acid . . 1·0   ,,
Water . . . . 6·0 parts.

This mixture, however, is not so advantageous in batteries without clay cells. According as the green di-acid or the blue tri-acid modification of chromium oxide is produced, the electrolytic decompositions which occur may be arranged in one or other of the following schemes, in which the original constituents are placed to the left, and the products of electrolytic decomposition to the right :—

$$
\left.
\begin{array}{l}
\text{1.}\\
K_2O . Cr_2O_6\\
3Zn\\
6H_2O.SO_3
\end{array}
\right\}
=
\begin{array}{l}
K_2O.SO_3,\\
3ZnO.SO_3,\\
Cr_2O_3.2SO_3.
\end{array}
\qquad
\left.
\begin{array}{l}
\text{2.}\\
K_2O.Cr_2O_6\\
3Zn\\
7H_2O.SO_3
\end{array}
\right\}
=
\begin{array}{l}
K_2O . SO_3,\\
3ZnO . SO_3,\\
Cr_2O_3 . 3SO_3.
\end{array}
$$
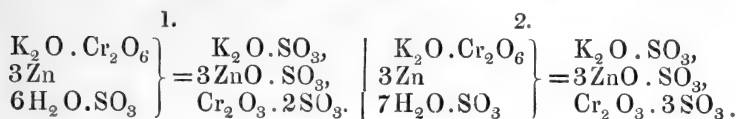
With the liquid employed by Warrington, in accordance with theory (viz. 1 equivalent of dichromate and 4 equivalents of sulphuric acid), the following reactions take place, if we suppose the clay cells to be removed and both exciting plates to dip into the chromic liquid :—

$$
\left.
\begin{array}{l}
\text{3.}\\
K_2O . Cr_2O_6\\
1\cdot714Zn\\
4H_2O . SO_3
\end{array}
\right\}
=
\begin{array}{l}
0\cdot429\,K_2O . Cr_2O_6,\\
0\cdot571\,Cr_2O_3 . 3SO_3,\\
1\cdot714\,ZnO . SO_3,\\
0\cdot571\,K_2O . SO_3.
\end{array}
$$
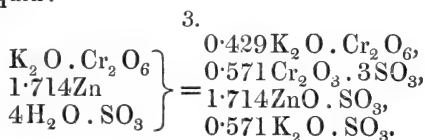
It is evident that in the liquids 1 and 2 the proportion between the constituents before and after decomposition remains unchanged until exhaustion of the battery, and that one of the primary conditions of a constant current is therefore fulfilled—but that, on the other hand, if Warrington's liquid be used without clay cells, the original conditions of current-production are no longer present as soon as the consumption of dichromate has reached 57 per cent. This waste, amounting to not less than 43 per cent., is accompanied by a yet greater disadvantage, viz. the fact that the acids present at the close of the action are not sufficient to form soluble salts with the bases. Deposits are soon formed on the exciting plates, whereby a polarizing action is induced which is attended with a diminution of the current. It is not, therefore, to be wondered at that the use of Warrington's liquid in the chromic-acid battery without clay cells gives very unsatisfactory results. Inasmuch as theory does not point out the influence exercised upon current-production by the formation of the green or blue modification of chromium oxide, nor tell the quantity of water to be added in order to ensure the best result, it appeared necessary to decide these points by experiment.

Ten liquids were prepared by the successive addition of measured quantities of sulphuric acid to Warrington's liquid; and from each of these, five liquids were prepared by the addition of increasing measured quantities of water. In certain of these liquids, systematically chosen, there was placed a single pair formed of amalgamated zinc and carbon, having a tangent-galvanometer in the connecting-circuit, care being taken that the conditions were the same in each case. The intensity of the current was then observed until the battery was almost exhausted.

The best working mixture was found to correspond with that

theoretically deduced in 1 above, and to be composed of the following proportions by weight:—

> Potassium dichromate . . 1 part.
> Hydrated sulphuric acid . 2 parts.
> Water . . . . 12 „

This liquid does not, when used, give rise to the formation of chrome alum; in contact with zinc it is coloured green; it dries up to a crystalline mass, which consists of sulphates of chromium, potassium, and zinc. By boiling with a large quantity of water a precipitate is formed having the composition $2\,Cr_2\,O_3\,.\,3SO_3.$ Zinc, even when impure, dissolves in this liquid without evolution of gas and with an extremely bright surface*.

In order to prepare 10 litres of this exciting liquid, 0·765 kilog. of commercial pulverized potassium dichromate (which usually contains 3 per cent. of impurities) are added, with constant stirring, to 0·832 litre of sulphuric acid, sp. gr. 1·836; when the production of chromic acid and potassium sulphate is complete, 9·2 litres of water are poured, in a thin stream, into the mixture. The liquid thus becomes gradually warmer, and the crystalline precipitate is entirely dissolved. In the researches to be described, the exciter for this liquid consisted of a rod of the *densest* gas-coal, 4 centims. broad and 1·3 centim. thick, and a rolled zinc plate 4 centims. broad by 0·5 centim. thick, the latter being covered with a film of wax, with the exception of the surface turned towards the coal rod; this surface was amalgamated. The zinc and coal were each immersed to a depth of 12 centims. in the liquid. The distance between the coal and zinc varied, according to circumstances, from 3 to 10 millims. Rather unsatisfactory results, as regards duration and constancy of current, are obtained with this liquid if cells are employed of the shape and size ordinarily used in Grove's or the zinc-carbon battery. This is accounted for by the fact that in the nitric acid of those batteries there is stored up a much larger quantity of oxygen, which may be used for depolarization, than in an equal weight of the chromic liquid, and that therefore a proportionately much larger quantity of the latter must be used in order to produce an equal effect. The cells of the chromic-acid battery must therefore be of three or four times the capacity of those of Grove's battery. The best shape for these cells is that of high narrow cylinders, which extend over no greater surface than equally powerful elements of the ordinary clay cells. The column of liquid, containing about 1·6 litre, reaches to a height of 0·28 metre, and has a diameter of 0·088 metre. The zinc-coal pair is immersed in the liquid to half its height only, and presents an

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* This liquid is very useful for removing the rust from metals.

active zinc surface of about 48 square centims. Fig. 2, Plate II. shows a battery of four such elements. If this battery be closed with a connecting wire of low conduction-resistance, the formation of a dark-coloured film of liquid, extending outwards from the dissolving zinc plate, is observed; this dark liquid gradually sinks, and at last collects, in the form of a well-defined layer, at the bottom of the glass cylinder. The original liquid has a specific gravity of $1\cdot140$, while the liquid laden with zinc sulphate which collects at the bottom has a gravity of $1\cdot272$. While the latter liquid sinks gradually downwards, its place is continuously supplied by fresh quantities of liquid which have not undergone electrolysis; constant circulation is hereby ensured, upon which the constancy of the current greatly depends.

I have not considered it superfluous to inquire how far the proportion of 1 equivalent of free chromic acid to 6 equivalents of free sulphuric acid is maintained in the liquid while undergoing exhaustion by electrolysis. The exhaustion of the liquid was allowed to proceed until the absolute intensity of the current had diminished from 36 to 6; for this purpose the battery was closed by a connecting wire of not very great resistance during a period of fourteen days. The composition of the liquids, referred to an equal weight of hydrated sulphuric acid, was as follows (A represents the original, and B the exhausted liquid) :—

A.			B.			
$H_2O.SO_3$	$100\cdot0$,	$2\cdot041$ equiv.	$H_2O.SO_3$	$100\cdot0$,	$2\cdot0410$ equiv.	$=a.$
$K_2O.Cr_2O_6$	$50\cdot2$,	$0\cdot340$,,	Cr_2O_6	$4\cdot5$,	$0\cdot0447$,,	$=b.$
			Cr_2O_3	$21\cdot7$,	$0\cdot2833$,,	$=c.$
			K_2O	$15\cdot9$,	$0\cdot3383$,,	$=d.$
			ZnO	$34\cdot0$,	$0\cdot8395$,,	$=e.$

If the electrolytic process occurs in the battery in the manner already supposed, then the following equations must nearly hold good :—$e=3c$ and $6b=a-(e+d+2c)$; *i. e.* for every equivalent of reduced chromic acid (Cr_2O_6) three equivalents of zinc are dissolved, and the proportion between the two acids yet combined with bases during electrolysis of the liquid remains nearly constant. That this is really the case appears from the following numbers :—

$$3c=0\cdot850, \qquad 6b=0\cdot268,$$
$$e=0\cdot840, \quad a-(e+d+2c)=0\cdot297.$$

If all the bases present be supposed to be in combination with sulphuric acid, then the proportion of free sulphuric to free chromic acid is in the original liquid $1 : 0\cdot424$, and in the nearly exhausted liquid $1 : 0\cdot320$. In the last instance, the rather too small value actually found for chromic acid may be accounted for by the formation of a small quantity of mercurous chromate by

the action of the mercury used for amalgamating on the chromic acid.

The constants of the battery were determined with a tangent-galvanometer on which one minute could be accurately read off. The current-intensity was determined, according to Gauss's measurement, by help of the formula

$$J = \frac{RT}{2\pi} \cdot \frac{\sin u}{\cos (u + x)}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

where $u =$ the angle described by the needle measured from zero of the circle,

$R =$ the radius of the galvanometer-ring in millimetres,

$T =$ the horizontal components of the earth's magnetism in Gauss's measure,

and $x =$ the deviation of the zero-point of the needle from the plane of the coil.

In order to arrive at the value of x, the angle u through which the needle is turned from its zero-point by a constant current, as also the angle v described by the needle under the influence of the same current reversed, must be determined; the equation then becomes

$$\tan x = \tfrac{1}{2}(\cot v - \cot u).$$

From the observations

$$\begin{array}{ll} u & 10^\circ\ 45', \\ v & 10\ \ 35, \\ u & 10\ \ 41, \\ v & 10\ \ 31, \\ u & 10\ \ 37, \end{array}$$

the value $1^\circ\ 58'$ was found for x; R was equal to 201·6 millims.; T was determined for a position not free from iron, by the decomposition of water, with help of the electrochemical equivalent of water, from the equation

$$T = \frac{2\pi}{R} \cdot \frac{\cos (u - x)}{\sin u} \cdot \frac{G}{at},$$

where $a = 0·009421$ milligrm., the electrochemical equivalent of water, G the amount of water (expressed in milligrammes) decomposed during the flow of the current for t seconds, and u the average deviation-angle of the needle during the water-decomposition. Two experiments gave the following values:—

$$\begin{array}{ll} t\ \ 1382''·0 & 1320''·0, \\ G\ \ 300·8\ \text{milligrms.} & 299·9\ \text{milligrms.}, \\ u\ \ 20^\circ\ 35' & 21^\circ\ 27'. \end{array}$$

From these observations the value 1·939 (mean of 1·941 and

1·938) is deduced for T. Formation of ozone and hydrogen peroxide during electrolysis, as also uncertainty in reading-off the position of the needle, were obviated by amalgamation and subsequent heating of the platinum plates of the decomposition-cell, which contained only a few grammes of water, and, further, by beginning to collect the mixed gases only after decomposition had proceeded for ten minutes, by which time the current had become perfectly regular. The gases were collected, at a constant pressure, in a measuring-flask over water previously saturated with the same gases. After observation of the volume of gas, the pressure, and the temperature, the measuring-flask was carefully removed from the water and weighed with the film of water which adhered to it, again filled with water to the point read off in measuring the gases, and again weighed. From these data the weight of water decomposed in the observed time t was calculated. The deviation-angle of the needle altered very regularly—in the first experiment only $7'$, in the second $9'$. From the whole of these estimations the following equation was deduced, and was used in calculating the intensity of the current, in absolute measure, throughout the series of experiments,

$$J = 62\cdot23 \cdot \frac{\sin u}{\cos (u - 1^\circ 58')}.$$

The chromic-acid battery without clay cells is the least-constant of the ordinary constant batteries. The curves in fig. 1 represent the current-diminution, A, of the ordinary carbon-zinc battery, and, B, of the chromic-acid battery without clay cells, in minutes and hours according to absolute measurement. The apparent breaks in curve B indicate the times at which the zinc plates were amalgamated. The current becomes constant much sooner with the carbon-zinc or with Grove's than with the chromic-acid battery, because the depolarization is much more complete in the first than in the latter instance: this is shown by the fact that, on shaking the chromic liquid, the current which had become constant is considerably increased. If that portion of the liquid which is in contact with the zinc plate be alone set in motion, the increase of the current does not occur; polarization takes place therefore only on the coal, not on the zinc plate.

The electromotive force of the chromic-acid battery without clay cells considerably surpasses that of any of the ordinarily used apparatuses. The amount of this electromotive force may be estimated within tolerably narrow limits without exact measurement: it must be greater than that of Grove's battery, consequently greater than 18·5 ; for the current of a chromic battery is able to overcome that of a Grove's battery, but not that of two Daniell's cells : the electromotive force must therefore lie between

21·3 and 18·5.   In order to arrive at a more exact number, the current-intensity of the battery J was determined; and after intercalation of a conduction-resistance l (expressed in British-Association measurement), this number J was again determined. The electromotive force e and the conduction-resistance of the cell ω are therefore expressed in absolute measurement as follows :—

$$e = \frac{Jl}{J_1 - J} J_1, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$\omega = \frac{Jl}{J_1 - J}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The observations were carried out in the following manner. The intensity of the current after it had become stationary was first of all determined from the mean value of five needle-observations made at equal time-intervals; these observations were repeated after the intercalation of the resistance l; and the two series, without and with the resistance, were then a second time repeated.   The four mean angle-readings, i_1, i_2, i_3, i_4, were thus obtained.   By the help of formula (1) the value of J was calculated from i_2, that of J_1 from the mean of i_1 and i_3; a value for J and J_2 was again deduced from i_3 on one hand, and from i_2 and i_4 on the other.

The electromotive force e and the conduction-resistance ω of the ordinarily employed batteries, and also of the chromic-acid battery, are given in the following Table, along with the observations upon which these values are based :—

	Silver-chloride battery.		Daniell's battery.		Grove's battery.		Zinc-carbon battery.		Chromic-acid battery.	
l	1·258	1·258	1·258	1·258	0·5126	0·745	1·258	1·258	1·258	1·258
J_1............	19·40	18·83	11·64	11·79	33·33	29·64	66·55	66·22	51·01	51·00
J	4·55	4·22	4·90	4·94	17·33	13·47	12·09	12·07	12·45	12·65
e	7·47	6·85]	10·65	10·69	18·51	18·40	18·59	18·57	20·72	21·16
Mean value of e	7·16		10·67		18·45		18·58		20·94	
ω	0·385	0·364	0·915	0·907	0·555	0·621	0·279	0·280	0·406	0·415

From these experiments it is evident that the chromic-acid battery without clay cells possesses an electromotive force greater, by about 13 per cent., than that of Grove's or the carbon-zinc battery.   On observing the valve of e from time to time, a period is noticed during which this value increases while the battery is steadily undergoing exhaustion.   By causing an unexhausted chromic battery to act against a partially exhausted battery, it becomes evident that the increase in the electromotive force is,

under the above conditions, apparent and not real. Polarization is therefore not prevented in the chromic battery to so great an extent as in the Daniell or Grove battery ; and the conditions on which equations (2) and (3) are based are not rigidly fulfilled during the increasing exhaustion of the exciting fluid. The extraordinarily large value of *e* for the chromic battery cannot, therefore, be received without doubt. That this value, however, is not due to errors of experiment is evident when we find that an equally high value is derived from an estimation of the electromotive force by Lipmann's electrometer in the unconnected battery. Experiment gave the proportion between the electromotive force of the chromic battery and of Grove's as follows :—

$$\frac{\text{Grove}}{\text{Chromic battery}} = \frac{18 \cdot 45}{21 \cdot 18}.$$

The chromic battery was allowed to remain at rest for a little time before the experiment. A solution of potassium dichromate and sulphuric acid, in those proportions which we have seen to be the best, is possessed of considerably lower conduction-power than the sulphuric and nitric acids used in the Grove cell. This disadvantage, however, is completely counterbalanced by the absence of the proportionally large conduction-resistance caused by the clay cells of the ordinary batteries, and by the small distance which may therefore be maintained between the exciting plates.

For the purpose of comparison, a couple, consisting of a coal rod and zinc plate (covered on the back with wax and amalgamated on the other side) was placed, first in a Grove's battery with clay cells, and then in a chromic battery without clay cells— the distance between the carbon and zinc being 15 millims., and the surface exposed 400 square millims., in each case.

The conduction-resistance of the two elements was as follows:—

Grove's battery with clay cells .   = 0·6401 B.A.
Chromic battery without clay cells = 0·5575 ,,

That a just view of the economical effect of the chromic battery may be attained, it will be necessary to examine somewhat closely into the chemical processes which take place in this battery. In an unconnected freshly filled Grove's battery the zinc-consumption is extremely small; it is only when, after long use, an electrolytic and endosmotic exchange has been established between the exciting liquids, that a consumption of zinc independent of the production of the current becomes marked. In an unconnected chromic battery, on the other hand, the consumption of zinc is the same as is required for current-production in the connected battery. In order to avoid this waste of

zinc, it is necessary that a form be given to the chromic battery such as shall render possible the removal of the plates from the liquid each time the current is broken. Especial interest attaches itself to a comparison of the consumption of zinc during current-production with that which takes place in the unconnected battery, not only from a practical but also from a theoretical point of view, inasmuch as theory alone affords no basis for deciding the question whether zinc dissolved in the unconnected is partly, wholly, or not at all used for current-production in the connected battery.

In order to decide this question, two exactly similar elements —one connected and having a tangent-galvanometer in the circuit, the other unconnected—were immersed to an equal depth in the same chromic liquid; the quantities of zinc dissolved in each case were then estimated at equal time-intervals, while the current-intensity of the connected elements was determined every fifteen minutes. The results of these experiments are contained in the following Table, where column I. contains the times of observation in hours (t), column II. the corresponding current-intensities J in B.A. measurement, column III. the zinc-consumption V_0 in the unconnected element, column IV. the consumption of zinc, V_e, theoretically required for the current-production—calculated from the electrochemical equivalent of zinc (0·03402),—column V. the actual consumption of zinc V_g (expressed in grammes) in the connected element.

I.	II.	III.	IV.	V.	VI.
t.	J.	V_0.	V_e.	V_g.	
1	56·0	3·000	6·860	9·268	26·0
2	54·5	5·730	13·536	18·069	25·1
3	52·5	8·381	13·967	26·102	23·5
4	50·7	10·913	26·177	24·623	24·4
5	48·5	13·165	32·117	42·033	23·6
6	44·5	15·326	37·568	48·336	22·3
7	41·0	17·468	42·591	54·434	21·8
8	38·0	19·305	47·237	60·531	22·0
9	35·0	20·966	51·524	65·481	21·3
10	32·7	22·508	55·529	70·646	21·4
11	30·6	23·821	59·277	75·646	21·6
12	28·3	25·095	62·743	79·926	21·5
13	26·4	26·347	65·976	84·328	21·8
14	24·7	27·569	69·001	87·946	21·6
15	23·0	28·779	71·818	91·827	21·8
16	21·0	30·021	74·390	94·973	21·7
17	19·0	31·221	76·718	97·709	21·5
18	18·0	32·461	78·923	100·520	21·5

From columns III. and IV. it is evident that, under the conditions of the experiments, rather less than half of the quantity

of zinc theoretically required for current-production in the connected battery is dissolved in the battery when unconnected, and that only a part of the metal dissolved in the latter case is employed for current-production in the former. These facts are the more worthy of remark when it is remembered that the chemical processes accompanying the solution of zinc are altogether the same in the open as in the closed battery; in neither case does polarization take place on the zinc plate.

These facts are entirely in keeping with the view which regards solution of zinc not as the cause, but as a necessary condition of the current. The numbers in column VI. express, in percentages of the total quantity of zinc dissolved per hour, the proportion of zinc lost, *i. e.* not used in current-production. The amount of zinc thus lost, amounting to about 22·5 per cent., remains tolerably constant during the exhaustion of the battery. The amount of nitric acid which passes through the clay cell of the Grove or carbon-zinc battery by *endosmotic* action is but small; with an amalgamated zinc plate of 156 square centims. surface in contact with sulphuric acid diluted with fifteen times its weight of water, the amount of zinc hourly lost amounted to only 0·3 grm.; in practice this might be overlooked. On the other hand, the best-constructed batteries, made on the above principles, when employed for the production of long-continued currents, show a very considerable consumption of zinc, dependent upon the formation of the current, and brought about by the *electrolytic* carrying over of nitric acid. In order to arrive at a knowledge of the amount of zinc lost in these batteries as compared with the loss in a chromic-acid battery, a small voltameter was placed in the connecting-circuit of a four-pair carbon-zinc battery: the loss of weight in the voltameter, which was connected with a calcium-chloride tube in order to keep back water vapour, was determined after the mixed gases had been replaced by dry air. The level of the nitric acid in the clay cells (which were new and of the best quality) stood somewhat under that of the sulphuric acid in the zinc cells; the capacity of the clay cells was 120 cubic centims., of the zinc cells 250 cubic centims., and the active amalgamated zinc surface encompassing the clay cylinders extended to 156 square centims.

If w be the amount of water, in milligrammes, decomposed in the voltameter during t seconds of time, z the loss of weight, in milligrammes, suffered by each of the four zinc cylinders during the decomposition, and a the electrochemical equivalent of water $= 0·009421$, then the mean current-intensity during the experiments is found in B.A. units by the equation

$$\mathbf{J} = \frac{w}{at};$$

and the amount of zinc, Z_0, required for the current-production during the experiments, by the equation

$$Z_0 = \frac{Zn}{H+O}\, w.$$

In experiments carried out by Dr. Gabriel the following values were obtained :—

$$w = 2714,$$
$$t = 14400,$$
$$J = 20 ;$$

hence

$$Z_0 = 9802.$$

The actual loss of weight for each zinc cylinder was, in milligrammes,

$$\begin{array}{r} 18129 \\ 16972 \\ 20481 \\ 19221 \\ \hline \end{array}$$

Mean . 18701

While, therefore, in the chromic-acid battery, arranged as already described, the loss of zinc averaged 22 per cent., in the nitric-acid battery employed in these experiments the mean loss amounted to 48 per cent.

The economical superiority of the chromic battery for regular work is confirmed by the results obtained in practice.   I am possessed of a battery of 40 pairs with an active zinc surface of only 40 square centims. in each plate ; by a simple handle-arrangement the plates are immersed in and withdrawn from the liquid when required.   This battery has served for all lecture experiments throughout eight semestres, during which time neither the zinc plates, the covering of wax, nor the exciting liquid has been renewed, nor have the conducting connexions been cleaned ; it has only been necessary occasionally to bestow a few minutes' labour upon the renewal of the amalgamation of the zinc plates, and from time to time to replace the water lost by evaporation. The apparatus, to this day, gives an electric arc between carbon points—not, it is true, sufficiently steady for optical projection, but perfectly serviceable for photochemical lecture experiments. The currents obtained from this battery after four years' use are yet, and are likely for some time to remain, strong enough for electrolytic demonstration, production of spark-spectra, decomposition of gases by induction-sparks, &c.   I need scarcely repeat

that effects of such magnitude can only be hoped for by paying strict attention to the easily observed rule of never allowing the pairs to remain in contact with the liquid one moment longer than the conditions of the experiment absolutely require.

In fig. 2 is shown the special disposition of the battery for spark-production. The apparatus consists of four pairs fastened to the frame *a,* which are immersed in and withdrawn from the liquid in the glass cells by means of the handle *b.* The pivots *e, e* of the frame *a* slide in the grooves *c, c* of the larger stand ; the pin *f* prevents the elements from sinking beneath this fixed point into the liquid. In order to allow of easy removal and replacement from the liquid, the movable part of the apparatus is nearly counterbalanced by the weight *g.* The zinc plates are soldered to the thin copper plate *h,* against the other *platinized* end of which the coal rod is pressed by means of a screw-clamp.

The amalgamation of the zinc plate is renewed by bringing the amalgamation-vessel (fig. 3) containing the requisite amount of mercury and dilute sulphuric acid under the plate, and gently raising the vessel until the plate touches the bottom ; the mercury which flows out is caught in a small tray, with which the glass cylinder is closed when the battery is not in use. The pole-wires of the battery, *i, i,* of a somewhat spiral form, conduct the primary current, a branch of which puts the current-interruptor into action, to a Ruhmkorff's apparatus furnished with an induction-coil about $0\cdot2$ metre in diameter and $0\cdot5$ metre in length. The current herein induced is conducted to a spark-apparatus (fig. 4) standing before the slit of a spectroscope : *w* is a three-necked flask which serves as a support ; the induced current passes from the small mercury-cup *a* through the fine wire *b* to the carbon point *c,* which is supported on a platinum wire ; thence it passes as a spark to the other carbon point *c ,* and so to the second mercury-cup a_1, which is in connexion with the other end of the induction-coil. The platinum wires supporting the carbon points are sealed into glass tubes, which again so pass into holes bored in the corks *d, d* as to be easily turned on their axes ; the corks themselves are supported on glass rods so that they may be moved up and down or turned upon their axes. These various movements allow of the quick and exact disposition of the carbon points before the slit of the spectroscope. The observation of the spark-spectra is carried out as follows :—The eye being in proper position, the battery is brought into activity by the left hand, while the spark-apparatus, the carbon points of which have been once for all set at the proper height, is so adjusted by the right hand that the spectrum coincides with the scale in the telescope. The spark, rendered more vivid by means

of an intercalated Leyden jar, is allowed to pass horizontally across the perpendicular slit; the length of the spark between two *thick platinum* points is from 1 to 2 centims.

The carbon points, for the reception of the liquids to be tested, are prepared from the ordinary, but not too porous, drawing-charcoal. In order to render them conductors, a number of small charcoal rods are placed in a covered porcelain crucible, which is set in a larger clay crucible and completely surrounded with charcoal powder; the crucibles are then exposed to the most intense white heat for a considerable time. The small rods are now pointed by means of a lead-pencil-sharper; and the little cones thus formed are cut off with a fine watchmaker's saw. A supply of these cones sufficient for many years may be prepared without difficulty, inasmuch as one worker can easily make ready five hundred in one day. The silica, magnesia, manganese, iron, potassium, sodium, and lithium which may be contained in the points require to be removed; for this purpose about a thousand of the points are repeatedly boiled in a platinum dish, first with hydrofluoric acid, then with concentrated sulphuric acid followed by concentrated nitric acid, and finally with hydrochloric acid, each acid being successively removed by boiling with water. The carbon points are ready for use as soon as a hole, of the same size as the platinum supports, has been bored in the base of each by means of a fine three-edged borer. For each experiment new carbon points are placed on the platinum supports. The liquid to be examined is brought on to the points by means of a small capillary glass tube warmed, if necessary, by a small gas-flame.

A carbon cone weighs about 0·015 grm., and can absorb more than its own weight of liquid. The spark-spectra obtained with these cones are of long duration; so that, if the cone be thoroughly impregnated, a renewal of the liquid by means of the small capillary tube will not be necessary for a considerable time.

Carbon points saturated with liquids giving simple spark-spectra, and also those liquids themselves, should be arranged in special small glasses, so that the normal spectra may be available at any time for comparison.

The spectrum-apparatus which had served for our earlier observations* was used for the production of the spectra in these researches. A small mica plate which could be easily removed and cleaned was used as a protection to prevent particles of liquid from being carried into the slit.

[To be continued.]

* Phil. Mag. S. 4. vol. xxii. p. 505.

LI. *On Maps of the World.* By GEORGE DARWIN, *M.A., Fellow of Trinity College, Cambridge**.

THE ordinary stereographic projection of the world in two hemispheres is utterly worthless as giving a true impression of the whole; for the linear scale at the margins of the circles is twice that at their centres. Its only merit is that there is no angular distortion. Mercator's projection gives a still more fallacious impression, except as regards the equatorial regions.

It appears to me therefore that there is a want, in the school-room and lecture-room, of some map which shall give a more truthful representation of the globe than the above, and which yet shall not be so expensive and cumbrous as a globe.

A gnomonic projection on to the faces of a regular icosahedron is but very slightly distorted, although a slight amount of angular distortion is here introduced. I have been told that at the recent Geographical Congress at Paris, some such projections as this were exhibited, and that they were of old date. Mr. Proctor has also made star-maps by projection on to the faces of a regular dodecahedron; but in 1872, when the idea occurred to me of using this projection, I was not aware of the fact.

If the icosahedral projection be developed and arranged as a band of ten triangles round the equator, with saw-like edges of five triangles in the north and five in the south, a very fair representation of the globe is given. And the interstices between the teeth of the saws may be arranged so as not to damage the continents very severely.

In this map the meridians are straight lines, but are broken in direction at the junction of two triangles. The parallels of latitude become ellipses, which may be easily laid out by aid of a property of conic sections; viz. if a circular cone be placed with its vertex at the centre of a sphere, and a section made by a tangent plane to the sphere, the radius of curvature at the vertices of this conic section is constant for all tangent planes, and varies as the tangent of the semiangle of the cone.

Now in our map the ellipses are represented with sufficient accuracy by the circles of curvature at their vertices; and the radii of these circles may be taken direct with the compasses from a sector, as the cotangents of the corresponding latitudes.

Besides a map of this kind, I have also constructed a portable quasi-globe with this method of projection. The faces of the

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icosahedron are made to hinge together, so that the whole can
be packed flat in the form of a half-hexagon.   Such a globe
was exhibited at the British-Association Meeting at Bradford.
When mounted, the icosahedron circumscribed a sphere of
25 inches diameter.   This form of globe might doubtless be
constructed much cheaper than a truly spherical one, because
the framework would be ordinary carpentry, and the twenty map-
sheets might be printed flat like ordinary maps.

In 1872 I showed the above described maps and globe to
General Strachey; and he suggested that by cutting down the
iscosahedron in some way, a still more satisfactory projection
might be attained.   It then occurred to us that by truncating
the solid angles of the icosahedron, a solid figure of 32 faces
would be obtained, viz. 20 hexagons and 12 pentagons.

If the truncation be carried on by slices until the truncating
planes touch the sphere enclosed in the icosahedron, these
hexagons are not regular, but have two sets of three sides equal
to one another; a long side is always opposite to a short side.
If unity is the radius of the sphere, the long sides and short
sides are respectively ·4913 and ·3401.   The pentagons are
always regular; and at this particular degree of truncation the
side of the pentagon is ·4913, and a pentagon is therefore
always contiguous to the long side of a hexagon; whilst hexagons
are always contiguous along their short sides*.

This projection was utilized by having a sort of umbrella-like
stand, with a pentagonal face in the middle, surrounded by
five hexagons; or else with a hexagon in the middle, surrounded
by three pentagons and by three hexagons.   The maps were
drawn on 32 separate sheets; and the sheets required to represent
any part of the world were mounted on the umbrella.

By these means about one fifth of the globe is shown at
once; and thus the equivalent of a very large globe might be
used in a room of ordinary size.   The sheets may also be
conveniently kept, since they are all flat, and will lie one on
another.

The figures 1, 2, 3, 4 (Plate I.) show the forms of the various
map-sheets, together with the figures required for laying out the
meridians and parallels of latitude.   Besides those kinds shown
in the figures, there are two pentagons which close in the two
poles; but it is so easy to lay them out, that it does not seem

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* This leads me to observe that if the angles of any one of the regular
solids be truncated in this way, another one is ultimately produced.   The
20-hedron and 12-hedron, the 8-hedron and cube, and the tetrahedron and
tetrahedron are thus correlated.   This property is of course due to the fact
that the polar reciprocal of any regular solid is itself a regular solid.   It is
curious to observe the transitional forms as the slices are cut off the angles.

worth while to give a figure. The meridians on the equatorial faces converge so little that it is more convenient to set them out by finding two points through which they pass. The broken lines in the figures are merely constructional.

In order that the meridians and lines of latitude may fall symmetrically on each face, it is better to set them every 9° or 6°, instead of every 10 as is usually done. For the whole globe, there are required 10 equatorial hexagons, 10 equatorial pentagons (5 in N. and 5 in S.), and 2 polar pentagons.

This 32-faced figure is a very close approximation to the globe.

The Murchison Fund of the Geographical Society (£40) has been granted for carrying this scheme out practically; and a Committee has been appointed, of which General Strachey and Mr. Francis Galton are members. The scale is large, the polyhedron being designed to circumscribe a sphere of 10 feet diameter. The various sheets of the map are stretched on light wooden frames; and they can be hasped on to a kind of umbrella, of which the handle is held horizontal. It is expected that it will be finished shortly; and it will, I believe, be placed in the rooms of the Society.

Another somewhat similar plan has occurred to me, and seems to me preferable, at any rate for somewhat smaller globes than the one above referred to.

Suppose A B C to be one face of a regular icosahedron *inscribed* in a sphere (see fig. 5), and that we bisect the arcs of great circles subtended by the sides A B, B C, C A respectively in D, E, F. Then pass a plane through D E F, and three others through A E F, B D F, C D E respectively. The face A B C may be replaced by the equilateral triangle D E F and the three isosceles triangles A E F, B D F, C D E. If this be done with every face of the icosahedron, we have a solid figure of 80 faces—20 equilateral triangles, and 60 isosceles (but nearly equilateral) triangles—inscribed in the sphere. If we project the globe on to this surface, with the vertex of projection at the centre, we obtain an excellent approximation to the true globe.

Now this plan would be very complicated if it were necessary to have 80 different map-sheets. Fortunately, however, the form of the triangles makes it advantageous to have four sheets united together, viz. the equilateral triangle and the three isosceles ones which have replaced the face of the original icosahedron.

Fig. 6 represents one of these sets of four sheets when spread out flat. These four sheets may be printed from a single plate, and may be pasted on to quasi-triangles, such as A B C (fig. 6), which are hinged or creased along the lines D E, E F, F D.

If the scale on which it is carried out is sufficiently small to permit of the faces being made of cardboard, it would, I think, answer very well.   We should then have to select the five appropriate sheets (each comprising four faces) and mount them on the umbrella stand; the five sheets would then represent one quarter of the globe.

The map-sheets may be kept in a very small compass, because the isosceles triangles may be folded down over the equilateral triangles, as shown in fig. 7.

In the other scheme it requires six or seven sheets to represent nearly one fifth of the globe: but it has the countervailing advantage of permitting a greater choice of the region which is to be in the middle; for, by having two umbrella stands, we can either place a pentagon or a hexagon in the middle.

In the instrument as made for the Geographical Society, the same general framework serves for both umbrellas, which may be shifted with great ease.   It was this advantage in choice of the central region displayed which induced the Committee to prefer the original 32-faced polyhedron.

A similar construction may of course be applied to a dodecahedron inscribed in a sphere; and we thereby obtain a 72-faced surface, viz. 12 pentagons, and 60 obtuse-angled isosceles triangles. Here, as before, the map-sheets might be printed in sets of six, viz. a pentagon surrounded by five triangles; three map-sheets will then give one quarter of the globe.   In this figure the pentagons are so large compared with the triangles that the approximation to the sphere is not very close.

Models of the various plans explained were exhibited at Bradford; but, by an oversight, no abstract of this paper appeared in the British-Association Report.

LII. *On a new Vertical-Lantern Galvanometer.   By* George F. Barker, *M.D., Professor of Physics*[*].

DESIRING to show to a large audience some delicate experiments in magneto-electric induction, in a recent lecture upon the Gramme machine, a new form of demonstration galvanometer was devised for the purpose, which has answered the object so well that it seems desirable to make some permanent record of its construction.

Various plans have already been proposed for making visible to an audience the oscillations of a galvanometer-needle; but they all seem to have certain inherent objections which have prevented them from coming into general use.   Perhaps the most common of these devices is that first used by Gauss in

* Communicated by the Author, having been read before the American Philosophical Society, May 7th, 1875.

1827, and adopted subsequently by Poggendorff and by Weber, which consists in attaching a mirror to the needle. By this means a beam of light may be reflected to the zero-point of a distant scale, and any deflection of the needle made clearly evident. The advantages of this method are :—1st, the motion of the needle may be indefinitely magnified by increasing the distance of the scale, and this without impairing the delicacy of the instrument ; and, 2nd, the angular deflection of the needle is doubled by the reflection. These unquestioned advantages have led to the adoption of this method of reading in the most excellent galvanometers of Sir William Thomson. While, therefore, for purposes of research this method seems to leave very little to be desired, yet for the purpose of lecture demonstration it has never come into very great favour—perhaps because the adjustments are somewhat tedious to make, and because, when made, the motion to the right or left of a spot of light upon a screen fails of its full significance to an average audience.

Another plan is that used by Prof. Tyndall in the lectures which he gave in this country. In principle it is identical with that employed in the megascope : *i. e.* a graduated circle over which the needle moves is strongly illuminated with the electric light ; and then by means of a lens a magnified image of both circle and needle is formed on the screen. The insufficient illumination given in this way, and the somewhat awkward arrangement of the apparatus required, have prevented its general adoption.

A much more satisfactory arrangement was described by Professor Mayer in 1872[*], in which he appears to have made use, for the first time, of the excellent so-called vertical lantern in galvanometry. Upon the horizontal plane face of the condensing lens of this vertical lantern, Mayer places a delicately balanced magnetic needle ; and on each side of the lens, separated by a distance equal to its diameter, is a flat spiral of square copper wire, the axis of these spirals passing through the point of suspension of the needle. A graduated circle is drawn or photographed on the glass beneath the needle ; and the image of this, together with that of the needle itself, is projected on the screen, enlarged to any desirable extent. The defect of this apparatus, so excellent in many respects, seems to have been its want of delicacy ; for in the same paper the use of a flat narrow coil, wound lengthwise about the needle, is recommended as better for thermal currents. Moreover a year later, in 1873[†], Mayer described another galvanometer improvement, entirely different in its character. In this latter instrument, the ordinary astatic galvanometer of Melloni was made use of,

[*] American Journal of Science, S. 3. vol. iii. p. 414, June 1872; Phil. Mag. S. 4. vol. xliv. p. 25.

[†] American Journal of Science, S. 3. vol. v. p. 270, April 1873.

an inverted scale being drawn on the inside of the shade, in front of which traversed an index in the form of a small acute rhomb, attached to a balanced arm transverse to the axis of suspension of the needle, and moving with it. The scale and index were placed in front of the condensing lenses of an ordinary lantern; and their images were projected on the screen in the usual way by use of the objective. This instrument is essentially the same in principle as the mirror-galvanometer; but it cannot be as sensitive as the latter, while it is open to the same objection which we have brought against this—the objection of unintelligibility. In the hands of so skilful an experimenter as Mayer, it seems, however, to have worked admirably.

It was a tacit conviction that none of the forms of apparatus now described would satisfactorily answer all the requirements of the lecture above referred to, that led to the devising of the galvanometer now to be described, which was constructed in February of the present year. Like the first galvanometer of Mayer, the vertical lantern, as improved by Morton*, forms the basis of the apparatus. This vertical lantern, as constructed by George Wale & Co., at the Stevens Institute of Technology, as an attachment to the ordinary lantern, is shown in the annexed cut (fig. 1). Parallel rays of light, from the lantern in front of which it is placed, are received upon the mirror, which is inclined 45° to the horizon, and are thrown directly upward, upon the horizontal plano-convex lens just above. These rays, converged by the lens, enter the object-glass, and are thrown on the screen by the smaller inclined mirror placed above it. The upper face of the lens forms thus a horizontal table, upon which water-tanks, &c. may be placed, and many beautiful experiments shown. To adapt this vertical lantern to the purposes of a galvanometer, a graduated circle, photographed on glass, is placed upon the horizontal condensing lens. Above this, a magnetic needle, of the shape of a very acute rhomb, is suspended by a filament of silk, which passes up through a loop formed in a wire, stretched close

Fig. 1.
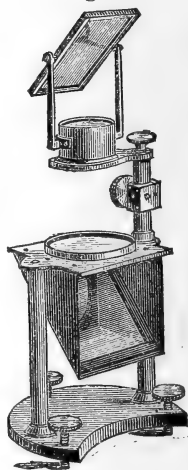
* Journ. Frank. Inst. S. 3. vol. lxi. p. 300, May 1871; Am. J. Sci. S. 3. vol. ii. pp. 71, 153, July & August 1871; Quart. J. Sci. Oct. 1871. In Duboscq's vertical attachment, which was advertised in his catalogue in 1870, the arrangement is similar, except that the beam received upon the mirror is a diverging one, and consequently the horizontal lens is of shorter focus; a total-reflection prism, placed above the object-glass, throws the light to the screen. The instrument gives a uniformly illuminated but not very bright field.

beneath the object-glass, and thence down to the side pillar which supports this objective, where it is fastened by a bit of wax, to facilitate adjustment. The needle itself is fixed to an aluminium wire, which passes down through openings drilled in the scale-glass, the horizontal lens, and the inclined mirror and which carries a second needle near its lower end*. Surrounding this lower needle is a circular coil of wire, having a cylindrical hollow core an inch in diameter, in which the needle swings, and a smaller opening transverse to this, through which the suspension wire passes. In the apparatus already constructed (in which the upper needle is five centimetres long) the coil is composed of 100 feet of No. 14 copper wire, and has a resistance of 0·235 ohm. The accompanying cross section (fig. 2) of the vertical-lantern galvanometer as at present arranged, drawn on a scale of $\frac{1}{12}$, will serve to make the above description more clear. A is the needle, suspended directly above the scale-glass D, by a silk filament, passing through the loop B, close under the objective C. This needle is attached to the aluminium wire *a b*, which passes directly through the scale-glass D, the condensing lens E, and the inclined mirror F at H, and carries, near its lower end, the second needle I. This needle is shorter (its length is 2·2 centimetres) and heavier than the upper one, and moves in the core of the circular coil J, whose ends connect with the screw-cups at K. This coil rests on the base of

Fig. 2.

the lantern, enclosed in a suitable frame. It is obvious that, when the instrument is so placed that the coil is in the plane of the magnetic meridian, any current passing through this coil will act on the lower needle; and since both needles are attached to the same wire, both will be simultaneously and equally deflected. Upon the screen is seen only the graduated circle and the upper needle; all the other parts of the apparatus are either out of the field or out of focus. Moreover the hole in
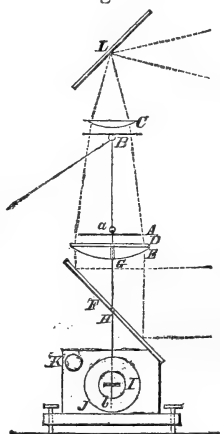
* After the new galvanometer was completed and had been in use for several weeks, I observed, in re-reading Mayer's first paper, a note stating that the idea had occurred to him of using an astatic combination consisting of two needles, one above the lens and the other below the inclined mirror—the two being connected by a stiff wire passing through holes in the condenser and the mirror. The plan of placing the coil round the lower needle does not seem to have suggested itself to him. Indeed it does not appear that the arrangement he mentions was ever carried into practical effect.

the lens is covered by the middle portion of the needle, and hence is not visible. The size of the image is, of course, determined by the distance of the galvanometer from the screen; in class experiments, a circle 8 feet in diameter is sufficient—though in the lecture above referred to the circle was 16 feet across, and the needle was 14 feet long, the field being brilliant.

The method of construction which has now been described is evidently capable of producing a galvanometer for demonstration whose delicacy may be determined at will, depending only on the kind of work to be done with it. In the first place, the needles may be made more or less perfectly astatic, and so freed more or less completely from the action of the earth's magnetism, and consequently more or less sensitive. Moreover an astatic system seems to be preferable to one in which damping magnets are used, since it is freer from influence by local causes—though, if desirable for a coarser class of experiments, the considerable distance which separates the needles in this instrument allows the use of a damping magnet with either of them. In the galvanometer now in use, the upper needle is the stronger, and gives sufficient directive tendency to the system to bring the deflected needle back to zero quite promptly. In the experiments referred to below, the system made 25 oscillations per minute.

Secondly, the space beneath the mirror is sufficiently large to permit the use of a coil of any needed size. Since, therefore, the lower needle is entirely enclosed within the coil, the field of force within which it moves may be made sensibly equal at all angles of deflection, as in the galvanometers of Sir William Thomson. Hence the indications of the instrument may be made quantitative, at least within certain limits. The circular coil, too, has decided advantages over the flat coil, since the mass of wire, being nearer to the needle, produces a more intense field. Were it desirable, a double coil, containing an astatic combination, could be placed below the mirror, the upper needle in that case serving only as an index. The instrument above described has a coil 3 inches in diameter and 1 inch thick, the diameter of the core being 1 inch. Since its resistance is only about a quarter of an ohm, it is intended for use with circuits of small resistance, such as thermo-currents and the like.

The results of a few experiments made with this new vertical-lantern galvanometer will illustrate the working of the instrument, and will demonstrate its delicacy. The apparatus used was not constructed especially for the purpose, but was a part of the University collection.

1. *Induction-currents.*—The galvanometer was connected with

a coil of covered copper wire, No. 11 of the American wire-gauge, about 10 centimetres long and 6 in diameter, having a resistance of 0·323 ohm. A small bar magnet, 5 centimetres long and weighing six and a half grammes, gave, when introduced into the coil, a deflection of 40°. On withdrawing the magnet the needle moved 40° in the opposite direction.

2. A small coil, 20 centimetres long and 3·5 in diameter, made of No. 16 wire and having a resistance of 0·371 ohm, through which the current of a Grenet battery, exposing 4 square inches of zinc surface, was passing, was introduced into the centre of a large wire coil, whose resistance was 0·295 ohm, connected with the galvanometer. The deflection produced was 20°. The same deflection was observed on making and breaking contact with the battery, the smaller coil remaining within the larger.

3. A coil of No. 14 copper wire, 60 centimetres in diameter, and containing about 40 turns, the resistance of which was 0·85 ohm, was connected with the galvanometer, and placed on the floor. Raising the south side 6 inches caused a deflection of 4°. Placing the coil with its plane vertical, a movement of 2 centimetres to the right or left caused a deflection of 3°, and of 20 centimetres of 10°. A rotation of 90° gave a deflection of 12°, and one of 180° of 24°. These deflections were of course due to currents generated by the earth's magnetism.

4. *Thermo-currents.*—Two pieces of No. 22 wire, 15 centimetres long, were taken, the one of copper, the other of iron wire, and united at one end by silver solder. On connecting the other ends to the galvanometer, the heat of the hand caused a deflection of the needle of 20°.

5. A thermo-pile of 25 pairs, each of bismuth and antimony, was connected to the instrument. The heat from the hand placed at 5 centimetres distance caused a deflection of 3°.

6. Two cubes of boiling water acted differentially on the pile. At the distance of 5 centimetres the deflection was 20°; moving one to 10 centimetres, the deflection was reduced to 5°.

7. *Voltaic current.*—A drop of water was placed on a zinc plate. While one of the connecting copper wires touched the zinc, the other was made to touch the water. The deflection was 16°.

The claim which is here made for the instrument, however, is rather for the general principle of its construction than for the advantages possessed by the individual galvanometer above described, which was constructed at short notice, to meet an emergency. The comparatively small cost for which it may be fitted to the vertical lantern, the readiness with which it may be brought into use, the brilliantly illuminated circle of light

which it gives upon the screen, with its graduated circle and needle, the great range of delicacy which may be given to the instrument by varying the coil and needles, so that all experimental requirements may be answered, and, finally, the satisfactory character of its performance as a demonstration galvanometer, all combine to justify the record which is here made of it.

Philadelphia, April, 1875.

LIII. *On a Differential Criticoid.* By Sir JAMES COCKLE, *F.R.S., Corresponding Member of the Literary and Philosophical Society of Manchester, President of the Queensland Philosophical Society, &c.**

1. **B**ESIDES the criticoids discussed in this Journal, at places to which my paper "On Primary Forms" in the Number for February last (1875) will give means of reference, there is another, which I call a differential criticoid. It indicates a certain relation between two differential equations, quantoids, or quotoids, whereof one is a transformation of the other by change of the independent variable†.

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* Communicated by the Rev. Robert Harley, F.R.S.

† The synthetic solutions, which I gave in art. 6 of my paper "On Primary Forms," and the footnote thereto, admit of convenient developments. Let h denote the operation $\int_x \xi \int_x \eta$, with this restriction, that no arbitrary constants are to be added. And let k denote $\int_x \eta \int_x \xi$ with the same restriction. Then

$$P\int_x \xi \int_x \eta \, . \, 0 = \frac{C_\xi}{1-h} 1 + C_\eta \int_x \xi \frac{1}{1-k} 1,$$

the operands on the dexter being unity. By developing the fractions in the operators we obtain two series. And by a similar process we should obtain the synthetic solution of a terordinal as an aggregate of three analogous series. If, extending the notation of that article, we put $\dfrac{d^2z}{dx^2}=r$, then the most general equation reducible to one with constant coefficients by a change of the independent variable is

$$\frac{d^2y}{dx^2} - \left(\frac{r}{p} + 2ap\right)\frac{dy}{dx} + p^2 y = 0. \ldots\ldots\ldots\ldots \quad \text{(a)}$$

Here if we take $\xi = e^{2az}p$ and $\eta = -e^{-2az}p$, the form of $\psi(x)$ is suggested; for when $h\psi(x) = \psi(x)$, then $\psi(x)$ is a particular integral. Assume $\psi(x) = e^{mz}$; then, if we add no arbitrary constants,

$$\int_x e^{2az}p \int -e^{-2az}pe^{mz} = \int_x e^{2az}p\, \frac{e^{(m-2a)z}}{2a-m} = \frac{e^{mz}}{m(2a-m)} \cdots\cdots \quad \text{(b)}$$

Hence if $m(2a-m)=1$, or $(m-a)^2=a^2-1$, then e^{mz} will be a particular integral, and the complete integral will be

$$y = Ce^{(a+\sqrt{a^2-1})z} + C_2 e^{(a-\sqrt{a^2-1})z}, \ldots\ldots\ldots\ldots \quad \text{(c)}$$

2. Let $\overset{n-1}{\underset{r}{P}}$ represent the sum of the products of the natural numbers $1, 2, 3, \ldots n-1$ taken r at a time. Then

$$\overset{n}{\underset{r}{P}} = \overset{n-1}{\underset{r}{P}} + n \overset{n-1}{\underset{r-1}{P}}$$

and

$$\overset{n-1}{\underset{r}{P}} = \Sigma n \overset{n-1}{\underset{r-1}{P}} = \Sigma n \, \Sigma n \ldots \Sigma n$$

to r factors, each Σ operating on all that follows it. This agrees with Mr. Scott's result (Quart. Journ. of Math. vol. viii. p. 29).

3. Let $\overset{n-1}{\underset{r}{H}}$ represent the sum of all the homogeneous products of r dimensions which can be formed by the first $n-1$ natural numbers and their powers. Then

$$\overset{n}{\underset{r}{H}} = \overset{n-1}{\underset{r}{H}} + n \overset{n-1}{\underset{r-1}{H}} + n^2 \overset{n-1}{\underset{r-2}{H}} + \ldots + n^{r-1} \overset{n-1}{\underset{1}{H}} + n^r$$

$$= \overset{n-1}{\underset{r}{H}} + n \left(\overset{n-1}{\underset{r-1}{H}} + n \overset{n-1}{\underset{r-2}{H}} + \ldots + n^{r-2} \overset{n-1}{\underset{1}{H}} + n^{r-1} \right)$$

$$= \overset{n-1}{\underset{r}{H}} + n \overset{n}{\underset{r+1}{H}}.$$

Hence

$$\overset{n-1}{\underset{r}{H}} = \Sigma n \overset{n}{\underset{r-1}{H}} = \Sigma n \Sigma(n+1) \overset{n+1}{\underset{r-2}{H}} = \Sigma n \Sigma(n+1) \ldots \Sigma(n+r-1),$$

since

$$\overset{n+r-2}{\underset{1}{H}} = \Sigma(n+r-1).$$

This agrees in substance with Mr. Jeffery's theorem (ibid. vol. iv. p. 370), though not with Mr. Scott's statement of it (ibid. vol.

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unless $a=1$, in which case it will be $(Cz+C_1)e^z$. Again, take

$$\frac{d^2y}{dx^2} + \frac{2-a}{x}\frac{dy}{dx} - \frac{1}{x^2}y = 0 \ldots \ldots \ldots \ldots \quad (d)$$

Here we may put $\xi = x^{a-2}$ and $\eta = x^{-a}$; and $y = x^m$ is suggested as, and is, a particular integral when

$$m(m-a+1) = 1 \ldots \ldots \ldots \ldots \ldots \ldots \ldots \ldots \ldots \quad (e)$$

In the fourth line above equation (2) of the paper the second "$\triangle$" should be replaced by ∇; and in art. 13 thereof, at lines 7 and 11, *for* Æ *read* Æ², and at its penultimate line, for "$2\mathit{æ}$" read $\mathit{æ}$; and in line 2 of art. 15 expunge the external exponent, adding that the elliptic integral is of the first species. In art. 9, line 17, *for* "F" *read* A. The second *cæsura* of a terordinal gives log ($\xi^2\eta$).

viii. p. 26).   The theorem is, if $[1]^r = 1 \cdot 2 \cdot 3 \ldots r$,

$$\frac{\Delta^{n-1}}{[1]^{n-1}} 0^{n-1+r} = \overset{n-1}{\underset{r}{\mathrm{H}}}.$$

4. Let $n_r = \dfrac{n(n-1) \ldots (n-r+1)}{[1]^r}$, so that $\Sigma n_r = n_{r+1}$.   **Also**

let ξ and η each represent $\dfrac{d}{dx}$; but let ξ denote $\dfrac{d}{dx}$ operating on
a function of x, and let η denote it operating on a function of y.
Let $\mathrm{X} = \phi(x)$, and let the successive differential coefficients $\phi'(x)$,
$\phi''(x)$, &c. be denoted by X_1, X_2, &c. respectively.   Also let
$\Xi = \mathrm{X}^p \mathrm{X}_1^q \mathrm{X}_2^r \ldots$, and let σ denote a finite integration, with re-
spect to n, which when applied to $f(n)\Xi$ operates on $f(n)$ only, n
in Ξ being treated as a constant.   Also let $\theta = \sigma\xi$, no arbitrary
constant being added after the finite σ integration.   Let σ have
∇ for its inverse operation.

5.   It is known (see Camb. Math. Journ. vol. i. last page) that

$$\left(x \frac{d}{dx} \right)^n = x \frac{d}{dx} \Delta 0^n + x^2 \left(\frac{d}{dx} \right)^2 \frac{\Delta^2 0^n}{1 \cdot 2} + \&c. ;$$

or, S denoting an aggregate of terms,

$$\left(x \frac{d}{dx} \right)^n = \mathrm{S} x^r \left(\frac{d}{dx} \right)^r \frac{\Delta^r 0^n}{[1]^r}.$$

And Mr. Walton has shown (Quart. Journ. of Math. vol. ix. p.
356) that

$$\left(e^x \frac{d}{dx} \right)^n = e^{nx} \mathrm{S} \overset{n-1}{\underset{n-r}{\mathrm{P}}} \left(\frac{d}{dx} \right)^r,$$

$\dfrac{d}{dx}$ entering into every term of each aggregate.

6.   Now

$$\left(\mathrm{X} \frac{d}{dx} \right)^n y = \frac{\eta^{n+1} - \theta^{n+1}}{\eta - \theta} \mathrm{X}^n y, $$

the term $\theta^n \mathrm{X}^n$ being suppressed in the development of the dexter,
because $\dfrac{d}{dx}$ enters into every term.   The development, thus re-
stricted, is

$$(\eta^n + \theta\eta^{n-1} + \ldots + \theta^{n-1}\eta) \mathrm{X}^n y,$$

giving

$$\left(\mathrm{X} \frac{d}{dx} \right)^n y = \mathrm{S} \theta^r \mathrm{X}^n \frac{d^{n-r} y}{dx^{n-r}}.$$

Operate on either side with $X\dfrac{d}{dx}$, and the dexter becomes

$$S\left(X\theta^r X^n + X\frac{d}{dx}\theta^{r-1}X^n\right)\frac{d^{n-r+1}y}{dx^{n-r+1}}.$$

But

$$\frac{d}{dx}\theta^{r-1}X^n = \nabla\theta^r X^n\;;$$

and such dexter may be written, omitting the last factor,

$$SX(1+\nabla)\theta^r X^n = S\theta^r X^{n+1},$$

and therefore

$$\left(X\frac{d}{dx}\right)^{n+1}y = S\theta^r X^{n+1}\frac{d^{n-r+1}y}{dx^{n-r+1}}.$$

Thus, if the development holds for n, it holds for $n+1$ and is generally true. It holds for $n=1$; and I remark that

$$\theta X^n = \sigma n X^{n-1}X_1 = n_2 X^{n-1}X_1,$$

which vanishes when $n=1$, explains the suppression of $\theta^n X^n$.

7. Proceeding, we find

$$\theta^2 X^n = \sigma n_2(n-1)X^{n-2}X_1^2 + \sigma n_2 X^{n-1}X_2\;;$$

and since

$$\sigma n_2(n-1) = 3n_4 + n_3,$$

consequently

$$\theta^2 X^n = (3n_4 + n_3)X^{n-2}X_1^2 + n_3 X^{n-1}X_2.$$

8. Proceeding further, we have

$$\theta^3 X^n = \sigma[(3n_4+n_3)(n-2)X^{n-3}X_1^3$$
$$+ \{2(3n_4+n_3)+(n-1)n_3\}X^{n-2}X_1X_2 + n_3 X^{n-1}X_3].$$

Now

$$\sigma(3n_4+n_3)(n-2) = \sigma(15n_5+6n_4+4n_4+n_3) = 15n_6+10n_5+n_4.$$

Again,

$$\sigma\big(6n_4+n_3(n+1)\big) = \sigma(10n_4+4n_3) = 10n_5+4n_4.$$

Hence we have

$$\theta^3 X^n = (15n_6+10n_5+n_4)X^{n-3}X_1^3 + (10n_5+4n_4)X^{n-2}X_1X_2$$
$$+ n_4 X^{n-1}X_3.$$

9. Put $X=x$; then

$$\theta x^n = n_2 x^{n-1},$$

and

$$n_2 = \frac{\Delta^{n-1}0^n}{[1]^{n-1}} = \overset{n-1}{\underset{1}{P}},$$

results which agree with both the formulæ of art. 5.   Again,

$$\theta^2 x^n = (3n_4 + n_3) x^{n-2};$$

and

$$3n_4 + n_3 = \frac{\Delta^{n-2} 0^n}{[1]^{n-2}},$$

as a reference to Mr. Scott's valuable paper (ibid. vol. viii. p. 21) will show.   This result agrees with the first formula of art. 5. Again,

$$\theta^3 X^n = (15n_6 + 10n_5 + n_4) x^{n-3},$$

and (compare Mr. Scott, ibid. p. 22)

$$15n_6 + 10n_5 + n_4 = \frac{\Delta^{n-3} 0^n}{[1]^{n-3}},$$

which also agrees with the first formula of art. 5.

10. Put $x = e^x$; then

$$\theta^2 e^{nx} = (3n_4 + 2n_3) e^{nx} = \overset{n-1}{\underset{2}{P}} e^{nx},$$

and

$$\theta^3 e^{nx} = (15n_6 + 20n_5 + 6n_4) e^{nx} = \overset{n-1}{\underset{3}{P}} e^{nx}$$

(see Scott, ibid.) : these results agree with those of **Mr. Walton.** And we may now write

$$\theta X^n = \overset{n-1}{\underset{1}{P}} X^{n-1} X_1,$$

$$\theta^2 X^n = \overset{n-2}{\underset{2}{H}} X^{n-2} X_1^2 + (\overset{n-1}{\underset{2}{P}} - \overset{n-2}{\underset{2}{H}}) X^{n-1} X_2.$$

I remark that $\theta^2 X^2$ and $\theta^3 X^3$, like θX, vanish identically, the evaluation being made by substituting, for n, the values 2 and 3 in the several cases.

11. Next let

$$\frac{d^n y}{dt^n} + na \frac{d^{n-1} y}{dt^{n-1}} + n_2 b \frac{d^{n-2} y}{dt^{n-2}} + \&c. = 0, \quad . \quad . \quad . \quad (1)$$

and let t and x be connected by $t = \int \frac{dx}{X}$; then

$$\left(\frac{d}{dt} \right)^n = \left(X \frac{d}{dx} \right)^n.$$

In (1) change the independent variable from t to x by this last formula and divide the result by X^n. Writing the quotient in

the form

$$\frac{d^n y}{dx^n} + n\mathrm{A}\frac{d^{n-1}y}{dx^{n-1}} + n_2\mathrm{B}\frac{d^{n-2}y}{dx^{n-2}} + \&c. = 0, \quad . \quad . \quad (2)$$

we shall have the relations

$$\mathrm{A} = \frac{a}{\mathrm{X}} + \frac{n-1}{2}\frac{\mathrm{X}_1}{\mathrm{X}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$\mathrm{B} = \frac{b}{\mathrm{X}^2} + (n-2)a\frac{\mathrm{X}_1}{\mathrm{X}^2}$$

$$+ \left(3\,\frac{(n-2)(n-3)}{3\,.\,4} + \frac{n-2}{3}\right)\frac{\mathrm{X}_1^2}{\mathrm{X}^2} + \frac{n-2}{3}\frac{\mathrm{X}_2}{\mathrm{X}}.$$

This last expression shows the mode of derivation ; but it reduces to

$$\mathrm{B} = \frac{b}{\mathrm{X}^2} + (n-2)a\frac{\mathrm{X}_1}{\mathrm{X}^2} + (n-2)\left(\frac{3n-5}{3\,.\,4}\right)\frac{\mathrm{X}_1^2}{\mathrm{X}^2} + \frac{n-2}{3}\frac{\mathrm{X}_2}{\mathrm{X}}. \quad . \quad (4)$$

12. Now

$$\mathrm{A}^2 = \frac{a^2}{\mathrm{X}^2} + (n-1)a\frac{\mathrm{X}_1}{\mathrm{X}^2} + \left(\frac{n-1}{2}\right)^2, \quad . \quad . \quad . \quad . \quad (5)$$

and

$$\frac{d\mathrm{A}}{dx} = \frac{1}{\mathrm{X}}\frac{da}{dx} - a\frac{\mathrm{X}_1}{\mathrm{X}^2} + \frac{n-1}{2}\frac{\mathrm{X}_2}{\mathrm{X}} - \frac{n-1}{2}\frac{\mathrm{X}_1^2}{\mathrm{X}^2}. \quad . \quad . \quad (6)$$

Hence, λ and μ being indeterminates,

$$\frac{d\mathrm{A}}{dx} + \lambda\mathrm{A}^2 + \mu\mathrm{B} = \left(\frac{da}{dt} + \lambda a^2 + \mu b\right)\frac{1}{\mathrm{X}^2} + \mathrm{L}\frac{a\mathrm{X}_1}{\mathrm{X}^2}$$

$$+ \mathrm{M}\frac{\mathrm{X}_2}{\mathrm{X}} + \mathrm{N}\frac{\mathrm{X}_1^2}{\mathrm{X}^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where

$$\mathrm{L} = (n-2)\mu + (n-1)\lambda - 1, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$\mathrm{M} = \frac{n-2}{3}\mu + \frac{n-1}{2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

$$\mathrm{N} = (n-2)\left(\frac{3n-5}{3\,.\,4}\right)\mu + \left(\frac{n-1}{2}\right)^2\lambda - \frac{n-1}{2}. \quad . \quad . \quad (10)$$

13. Hence $\mathrm{M} = 0$ gives

$$\mu = -\frac{3}{2}\frac{n-1}{n-2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

and (11) combined with $\mathrm{L} = 0$ gives

$$\lambda = \frac{3n-1}{2(n-1)}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

and, with these values of λ and μ, the expression for N becomes

$$N = \frac{(1-n)(3n-5)}{2^3} + \frac{(n-1)(3n-1)}{2^3} - \frac{n-1}{2}$$

$$= (n-1)\left(\frac{5-1-4}{8}\right) = 0, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

and N vanishes identically.

14. Consequently, since $\frac{1}{X^2} = \left(\frac{dt}{dx}\right)^2$, we find that

$$\left(\frac{dA}{dx} + \frac{3n-1}{2(n-1)}A^2 - \frac{3}{2}\frac{n-1}{n-2}B\right)dx^2$$

is equal to

$$\left(\frac{da}{dt} + \frac{3n-1}{2(n-1)}a^2 - \frac{3}{2}\frac{n-1}{n-2}b\right)dt^2;$$

and either of these equivalent expressions is a differential criticoid, which we may call a differential quadricriticoid. The formulæ fail when $n=1$ or $n=2$, and primordinals and biordinals have not such a criticoid. The differential varies from the ordinary quadricriticoid in this, that the corresponding coefficients of the former contain n, the order of the given differential equation, while those of the latter are free from n.

"Oakwal" near Brisbane, Queensland,
    Australia, June 11, 1875.

LIV. *A new Relation between Electricity and Light: Dielectrified Media Birefringent.* (Second Paper.) *By* JOHN KERR, *LL.D., Mathematical Lecturer of the Free-Church Training College, Glasgow*[*].

WE come now to the case of liquids. The principal results are summed up in articles 33, 42.

25. *Plate-cell for Liquid Dielectrics.* A well-made cell is in these experiments indispensable. The piece which I call a plate-cell, though simple enough in plan, is rather troublesome to make, and very liable to fracture in the workman's hands; but it is the only cell which I have yet got to work satisfactorily, and I have used no other in my latest experiments. I begin, therefore, with a full account of its construction.

The reader will remember the large dielectric of plate glass (2 inches wide and 6 long) which formed the principal subject of my first paper. A new block of plate glass is taken similar to that one in form, size, and mounting; but the borings are different.

For convenience of description, I shall suppose the block already fixed across the two insulating pillars of glass, its long edges horizontal, and plate-faces vertical. The pillars are in this case about 6 inches apart, close to the ends of the block; they project also 2 or 3 inches above it.

A narrow tunnel, straight and clean, is worked through the block by drill and file at right angles to the plate-faces. The tunnel is concentric with the block; its two sides are sensibly plane and perpendicular to the long edges of the block; its floor and roof are slightly rounded. The length of the tunnel, or thickness of the plate, is $\frac{3}{4}$ of an inch, its width (horizontal) $\frac{3}{8}$ of an inch, and its height (vertical) about $\frac{3}{4}$ of an inch, so that there is a good margin of polished plate-surface all round each mouth. This tunnel, with its mouths properly closed, forms the cell; it is usually charged with liquid through a fine vertical boring which leads into it at the centre of the roof.

The fine borings for the induction-wires lie here, as they did in the dielectric of glass, midway between the plate-faces: but they are not now horizontal; they pass from the two upper corners of the block obliquely downwards, and enter the tunnel at the centres of its opposite sides.

The induction-wires are of copper, and about $\frac{1}{20}$ of an inch thick. Each wire is bent at about an inch from the end, and closely doubled upon itself, so as to present a very well-rounded extremity. This end of the wire enters from the boring a little way into the tunnel. At the outer end of the boring each wire is bent closely upon the block, and tied very firmly along several inches of its length to the upper part of the adjacent pillar. The parts of the two wires within the tunnel are easily bent by pincers into proper form and position (virtually fixed), so as to project from the sides of the tunnel towards its centre, in one line parallel to the long edges of the block, and to about $\frac{1}{8}$ of an inch from each other. To an eye looking straight through the tunnel, the induction-wires appear single, with perfectly rounded ends, exact images of each other, lying in one horizontal line, and projecting equally far from the sides of the tunnel.

Two small panes of plate glass, each about 2 inches square and $\frac{1}{16}$ of an inch thick, and of the best quality obtainable in the glass-shops, are placed on opposite faces of the block, so as to close the mouths of the tunnel with a good margin of contact between plate-surfaces all round. Outside of each plate is a square of india-rubber cloth of the same size. Outside of the cloths are two stiff planks, each 2 inches (horizontal) by 4 (vertical), which are connected above and below by nail and nut, so as to form a weak screw-press. The cloths and planks are per-

forated as in prolongation of the tunnel, or rather a little more widely, so as to allow distinct vision through the whole cell along the length of the tunnel. When the press is closed, the cell is perfectly tight. The surface of pillars and block is well varnished with lac, except the spaces covered by the closing-plates, which are kept clean. The outer ends of the borings, where the wires leave the block, are well closed with sealing-wax.

The cell is filled with liquid either through the central boring, already mentioned, or otherwise—more easily in many cases through a partially closed mouth of the tunnel; it is easily emptied by unscrewing the press, and allowing the liquid to run off upon one of the glass plates, loosened and properly directed. The only inconvenience (and a very serious one) which I have noticed in the working of this piece, is the difficulty of cleaning the cell perfectly when charging it with a new liquid.

26. *General Form of the Experiments.*—The plate-cell is charged with clean liquid. The electric force is obtained sometimes from the inductorium, but generally from an electric machine. In the former case the wires from the cell are connected with the terminals of the secondary coil; in the latter case one of the wires is inserted in the prime conductor, and the other is connected with a good discharging train. A horizontal beam of light, emitted by a narrow paraffin-flame, passes first through a polarizing Nicol, then through the plate of dielectrified liquid, and then through a second Nicol, which acts as an analyzing eyepiece. The eye is generally so placed as to see the flame through the centre of the cell, midway between the induction-terminals. The light traverses the liquid in a direction perpendicular to the closing-plates of the cell, and therefore perpendicular to the lines of electric force.

It will be observed that the arrangements and procedure are much the same as they were in the former experiments, with the dielectric of plate glass. The transparent dielectric is now a plate of liquid; and the electric forces applied are much less intense; but there is no other material change.

Bisulphide of Carbon.

27. The plate-cell is charged with bisulphide of carbon; the wires from the cell are connected with the knobs of the secondary coil; and all the other arrangements are as indicated above. The compound plate of thin glasses and bisulphide (placed in almost any light and towards any object) is as purely transparent to the eye as so much air; and when it is introduced properly between the two Nicols at extinction, it gives not

the faintest restoration, unless the closing plates are unduly strained by the screw press.

The primary circuit is closed, and the spark-terminals, placed at first in contact with each other, are drawn apart slowly till sparks pass through the liquid in the cell. The spark-terminals are then pushed a little nearer to each other, till the discharge passes wholly through the air; the primary circuit is broken, and the apparatus is ready for experiment. To obtain a good observation, the liquid should be very clean, the room somewhat darkened, and the light due midway between the terminals.

Plane of polarization at 45° to the horizon or to the lines of force: as soon as the primary circuit is closed, there is a very distinct flickering restoration of the light in the polariscope, the flicker keeping time with the oscillations of the rheotome. The cleaner the liquid plate, the more distinct is the effect; and when the liquid is sensibly without speck, the restoration of the whole image of the flame from pure initial extinction is admirably clear and delicate.

Plane of polarization at 0°, or at 90° to lines of force: generally a small effect in the polariscope, much fainter than in the former case, and tending to evanescence as the angular adjustments are improved throughout the part examined of the electric field.

This was the first successful observation which I made upon any liquid dielectric. All the following experiments are with the electric machine.

28. The plate machine employed is a small one, strong enough to send sparks through the liquid in the cell when it is worked vigorously. It will be remembered that the prime conductor is connected with earth by a copper wire, which is interrupted in the polariscope by $\frac{1}{8}$ of an inch of carbon disulphide.

The only difficulty in these experiments is to get the liquid clean enough. At the beginning of a series of observations the cell is cleaned thoroughly, washed with alcohol or other proper solvent, and rinsed repeatedly with ether, no cloth or other rubber being applied. The cell is then charged with fresh bisulphide, many times in succession if necessary, the liquid being introduced through a filtering-funnel which has had its tube drawn to a fine end. In this way specks of solid matter which are present in the liquid as obtained from the apothecaries, specks also of organic fibre or of other dust which would otherwise enter from the surrounding air, are kept out of the cell. But even when all precautions are taken, I find it very difficult to get the liquid specklessly clean. In many cases, as soon as the electric force begins to act, specks of dust formerly invisible

are attracted from the most distant parts of the cell, and show themselves at once about the centre of the field, either as a set of sparkling and irregularly moving points, or as a chain connecting the terminals and remaining always bright in the polariscope. But whatever the form which they assume, they interfere very materially with the principal effect, if they do not mask it altogether.

29. *First experiment.*—The room well darkened; the light a narrow flame immediately in front of the first Nicol; the two Nicols about 4 feet apart; the dielectric about a foot from the eye. Before extinction by rotation of the analyzer, the light is seen through the plate of liquid as a narrow and very well-defined vertical band, midway between the terminals.

Plane of polarization at 45° to the horizon. When the prime conductor is charged, the light comes out in the polariscope steadily and very clearly from pure extinction. When the prime conductor is discharged by a spark upon the knuckle, the restored light disappears instantly and totally. When the light is again restored by electric action, and a compensating slip of glass is introduced and compressed horizontally, the light is either extinguished or greatly weakened; when the compensator is extended horizontally, the light is always strengthened.

With one gentle turn of the plate, the light comes out continuously from initial extinction till, at the end of the turn, it is very well restored: in this case the extinction by compression of the compensator parallel to the lines of force is perfect.

With a strong electric action, with or without occasional sparks through the liquid, the light is much more strongly restored: in this case, compression of the compensator parallel to lines of force does not act so regularly, never extinguishes the light all at a time, but produces generally a very dark broad band, approximately straight and horizontal, which descends from the outer parts of the electric field towards the centre as the compression increases. This proves that the amount of birefringent action varies considerably throughout the part examined of the electric field. In the black band itself, the actions of the dielectrified bisulphide and horizontally compressed glass compensate one another; at other equatorial points of the field one or the other action predominates—that of the glass outside of the dark band, that of the liquid inside.

Plane of polarization at 0° or at 90° to lines of force. One gentle turn of the plate has generally no sensible effect, but sometimes gives a barely perceptible restoration from perfect extinction. A strong electric force gives generally a clear restoration from extinction; the effect, however, is not nearly so intense as in the case of 45°, and it tends to evanescence as the lines of

force are more nearly parallel or perpendicular to the plane of polarization throughout the part examined of the electric field.

30. *Second experiment.*—All the arrangements are as in the first experiment, except that the optical pieces are brought as close together as the stands will allow. The dielectric is only a few inches from the eye. The light is large and strong, and extends well above and below the level of the terminals. All the observations made in the first experiment are repeated, and with similar results.

The dark band given by the compensator is now more noticeable, and more regular in its appearance and movements.

If the liquid contains a few specks of dirt, this experiment succeeds better than the first, sometimes indeed gives a good result when the first fails altogether. The solid particles generally arrange themselves as a chain between the terminals. In the first experiment, the principal effect is obscured and often obliterated by the light from the chain; while in the second the light is generally well restored above and below the level of the terminals, the place of the chain being marked by a patch of more intense light, which is not affected by the compensator. In this experiment and the preceding, the dependence of intensity of optical effect upon intensity of electric action is very observable. When the insulation of the prime conductor is not very good, it requires little irregularity in the working of the machine to make the light clearly glow and fade again with each turn of the plate.

31. *Third and fourth experiments.*—The flame is turned broadside-on to the polariscope; and a screen is placed close to the lamp, which reduces the light to a narrow horizontal band: the arrangements are otherwise as in the first and second experiments. The results obtained are consistent with those already described, and add little to them. But as I would rather give too much here than too little, I add the final note which was taken at the time of observation.

The liquid very clean; the light reduced to a narrow and sharply defined horizontal band, encroached upon at both ends by the terminals; initial extinction in the polariscope perfect. At 45° one gentle turn of the plate gives a good restoration of the band from terminal to terminal, and the restored band is well extinguished by horizontal compression of the compensator: at 0° and at 90° the same electric force gives no sensible effect whatever. When the electric action is intense, the effects are somewhat similar: at 45° the restored band is very well extinguished by horizontal compression of the compensator, except at points quite close to the terminals; at 0° and at 90° the wires are tipped each with a narrow band of light, the intermediate part

of the electric field remaining dark. Hardly in any case of strong electric action is there absolutely no perceptible restoration of the band at the centre of the field; but the effect is always comparatively very faint at 0° and at 90°, unless the angular adjustments are very imperfect.

32. *Fifth experiment.*—The optical pieces close together, and the flame broadside-on to the polariscope. The light now fills the transverse section of the first Nicol, and extends well beyond the interterminal part of the electric field all round. When the principal sections of the two Nicols are at right angles to each other, there is a good initial extinction between and around the terminals.

Plane of polarization at 45° to the horizon: the electric action brings out a distinct illumination over a good extent of the field, very faint at the outer parts, but brightening towards the axis of the field, and rising into two patches of comparatively strong light at the terminals. When the compensator is introduced and compressed horizontally by a continuously increasing force, it produces extinction first in the outer equatorial parts of the field, then at the central parts; and then, after a considerable increase of compression, it extinguishes or greatly weakens the bright patches at the terminals.

Plane of polarization horizontal or vertical: the electric action brings out a narrow border of glowing light along the outlines of both wires, and a fainter illumination in other parts of the field, leaving a broad equatorial band very dark.

33. *Summary.*—Carbon disulphide is birefringent when dielectrified, acting upon transmitted light as glass extended along the lines of force. The electrostatic force and the birefringent power increase together: they also vanish simultaneously, the optical effect disappearing abruptly and totally at the instant of electric discharge (not through the bisulphide). Some irregularities are observed in the experiments when the electric action is intense; but these are certainly due in great part, if not wholly, to the want of even approximate uniformity through any considerable extent of the electric field.

34. The optical effect with which we are now concerned cannot be attributed to specks of dirt, however fine; for it improves in every respect as the liquid is made cleaner; and when any solid particles are visibly present, it improves for some distance outwards from the axis of the field. Neither can it be attributed to electric discharge; for the bisulphide is an excellent insulator, and any discharge which is known to pass through it in the course of the experiments is very sharply disruptive. The effect appears to be due simply to the action of electrostatic force upon the molecules of the liquid.

The character of the effect in the first and second experiments (29, 30) is of some consequence in this connexion. When the liquid is perfectly clean and the initial extinction pure, the light is brought out by electric action with remarkable distinctness and delicacy, and without a trace of unsteadiness or distortion. The whole surface of the visibly restored flame is continuous, and its outline beautifully sharp. These patent characteristics of the effect in the polariscope are strongly suggestive of some regular arrangement of the ultimate molecules of the bisulphide as the immediate cause of the phenomena. And the compensator confirms the suggestion, while it shows, further, that the new arrangement of the molecules either is in itself, or is at least in effect equivalent to, a positive uniaxal structure with its axis directed along the lines of force.

Of such a structure in the case of a liquid, I see no probable explanation except that already advanced in the case of glass. The particles of the bisulphide, when electrically polarized, throw themselves into files along the lines of force ; and the system of electric curves thus physically constituted gives the liquid a new structure, which is symmetrical at each point with reference to the line of force through the point. If we adopt Faraday's theory of electrostatic induction, I think that the common experiment of the magnetic curves affords rather a proof of the present theory than an illustration. Another illustration of the same kind is afforded by the interterminal chain of solid particles in the present experiments.

35. I may mention here that I found it very useful in some of the preceding experiments, and still more useful in some of the following, to draw sparks occasionally from the prime conductor while attending to the polariscope. The abruptness of the change thus produced in the field of vision adds remarkably to the delicacy of the observation.

Benzol.

36. The plate of liquid is very transparent, and an excellent insulator. A steady electric action of the requisite intensity is obtained from the plate machine. Ruhmkorff's coil is not suitable in this case, as a few of its powerful discharges through the cell are sufficient to contaminate the liquid with a fine deposit of carbon.

Here again the only difficulty is to get the liquid clean enough. A few specks of organic fibre or of other dust from the air of the room or elsewhere, if they get into the cell, are sure to obscure the principal effect extremely, if they do not quite mask it. When the specks are numerous enough, they form a chain between the terminals, as in the case of carbon

disulphide. In some plates the chain holds together permanently; and in such a case, if the chain is not very coarse, a good effect may generally be obtained in the parts of the field above and below the chain, as formerly in the second experiment upon carbon disulphide. In other plates the chain breaks up violently at the instant of discharge of the prime conductor upon the knuckle; and in such a case I have sometimes obtained a good effect by drawing sparks in rapid succession from the prime conductor. When the specks are so few and of such forms as not to give a chain, they still interfere noxiously, presenting themselves as a set of sparkling points, which dart hither and thither through the central parts of the electric field.

After numerous trials, many of them ineffective, I got at length some specklessly clean plates of benzol. The principal effects then presented themselves with beautiful distinctness and perfect regularity. The experiments were the same as those upon carbon disulphide already described, and were often repeated, particularly the first and second (29, 30); and the effects obtained were exactly similar in kind and equally distinct, though less intense. Without more details, I give here the final note which was taken at the time of observation.

Benzol.—Plane of polarization at 45° to the lines of force; strong electric action : the light is clearly restored from extinction, and is then extinguished perfectly by compression of the compensator in a direction parallel to the lines of force. Plane of polarization at 0° or at 90° to the lines of force; strong electric action, with or without occasional spark-discharge through the liquid : not a trace of effect in the polariscope, the extinction equally pure before and after the instant of strong spark-discharge from the prime conductor.

Paraffin and Kerosene Oils.

37. *Young's Paraffin Oil.*—This liquid is a strong insulator, and gives a very transparent plate. Its action is similar to that of benzol, but much weaker. Benzol, as I have just mentioned, gave me often a distinct principal effect above and below a fine interterminal chain of solid particles, but this liquid never. The action, indeed, is so faint that there was only one of several arrangements tried which gave a good regular effect.

Experiment.—All the pieces are laid as in the first experiment with carbon disulphide (29), except the dielectric, which is now placed close to the second Nicol, a few inches from the eye, so that no light is received from the lamp except through points of the electric field very near the centre. No result could be accepted as decisive till the liquid was perfectly clean; and the observations were carried out accordingly.

Plane of polarization at 45° to lines of force; strong electric action : the light comes out faintly, but very distinctly, from extinction, and is then extinguished by compression of the compensator parallel to the lines of force; vanishes abruptly at the instant of discharge of prime conductor.

38. *Kerosene Oil.*—This liquid is more purely transparent than paraffin, but not such a strong insulator. When the plate of liquid is specklessly clean, but not till then, the form of experiment just described in the case of paraffin succeeds perfectly ; and the effects obtained are similar, as might indeed be expected.

Plane of polarization at 45° to lines of force; strong electric action : the light is restored faintly, but very clearly, from pure extinction, and is then extinguished perfectly by compression of the compensator parallel to the lines of force. As in all the former cases, the restored light vanishes abruptly at the instant of discharge of the prime conductor.

Spirits of Turpentine.

39. Two samples of this liquid, of contrary photogyric powers, were mixed in such proportions as to give a plate inactive in the polariscope. The plate was very transparent and a good insulator. All the arrangements being as in the first and second experiments on carbon disulphide, and the liquid being at last quite clean, the action of the dielectrified turpentine in the polariscope was found to be perfectly regular, similar to that of benzol, but rather fainter, though not nearly so faint as the actions of paraffin and kerosene. Even when some specks of dirt were present, and formed an interterminal chain, there was sometimes a good effect obtained above and below the chain. I had occasion to notice particularly while working with this liquid, what I had already observed more distinctly in the case of paraffin, that it is sometimes of much consequence to receive the most of the light through the centre of the electric field.

Oil of Turpentine.—Plane of polarization at 45° to lines of force; strong electric action : the light is restored, faintly but very distinctly, from extinction, and is then extinguished by compression of the compensator parallel to lines of force. Plane of polarization at 0° or at 90° to lines of force, strong electric action : no perceptible effect in the polariscope.

Olive-Oil.

40. This liquid is, I believe, the best insulator among the heavy oils ; and although coloured, it gives a beautifully transparent plate. I have made a good many experiments upon it, at several times, finding the material difficult to manage, and trying to assure myself perfectly of the interesting fact that its

action is contrary to the actions of the five preceding liquids. The material employed latterly was the best olive-oil to be had from the apothecaries.

The oil was let into the cell through filtering-papers, which probably gave up some of their finest fibres to the thick and adhesive liquid; and one or two of these fibres, though very fine and short, were enough to mask the principal effect. From very small fibres I could never cleanse the liquid perfectly till I applied a simple device for removing them by electric action, after the cell had been closed and filled. Two fine wires, properly insulated from each other near their ends by fine (vaccine) tubes of glass, were let down as one piece through the central boring into the cell, one wire being connected with the prime conductor of the slowly worked machine, and the other wire uninsulated : the fine fibres were at once attracted, and arranged themselves as a chain or woof between the bare ends of the two wires, which were then withdrawn as one piece without being discharged. It was found unsafe to apply this piece a second time. When a second application was required, a new piece had to be made of clean materials.

The oil was in this way cleared of all visible specks of solid matter; but it did not yet work well when examined under electric action in the polariscope. Sparkling points appeared in all parts of the electric field, darting hither and thither irregularly. These were fine air-bubbles, produced either by the preceding operations upon the liquid, or by occasional spark-discharges through the cell : they generally rose out of the way more or less completely in a few minutes. Accordingly it often happened in these experiments, that a plate of oil which gave no trace of the principal effect at first, acted very well after it had been left undisturbed for some minutes, giving the effect at last with perfect distinctness and regularity. But if the liquid was disturbed anew by accidental discharge through the cell, things were at once in much the same condition as at first.

I may mention, finally, that in the course of the experiments, whenever the electric force reached any considerable intensity, distinct movements were generally observed in the oil. They bore no resemblance to the current-motions which are observed under similar conditions in alcohol and other imperfectly conducting liquids; they rather presented the appearance of pulsations, quick and irregular heavings of the liquid. Whatever the nature of these movements, and whatever their relation to the electric force, I see no reason to think that they had any influence on the principal effect.

All the experiments on carbon disulphide were repeated with olive-oil, the first (29) being most frequently repeated and espe-

cially depended upon. In my last series of experiments upon this liquid, I carried on the observations with unrelaxed care for several days, till the effects came out with perfect regularity. The action was found to be, as distinctly as possible, contrary to that of carbon disulphide, and equal, or very nearly equal, to that of benzol. I give here the final note which was taken at the time of observation.

Olive-Oil.—Plane of polarization at 45° to lines of force; strong electric action : the light is restored very distinctly from extinction, and is then extinguished regularly and perfectly by tension of the compensator parallel to the lines of force, and strengthened always by compression in that direction. At 0° and at 90° not a trace of effect in the polariscope.

Castor-Oil.

41. The liquid plate is very clearly transparent, and acts as a weak photogyre, separating the blue and red perceptibly in the polariscope. This oil is not nearly such a good insulator as the preceding, and has generally current-movements produced in it by the electric action ; and accordingly the results of experiment upon this liquid were very irregular and imperfect. The arrangements were generally the same as in the first experiment on bisulphide of carbon (29).

Plane of polarization at 45° to lines of force : the light is restored strongly from good extinction, and is then either pretty well extinguished or greatly weakened by tension of the compensator parallel to the lines of force, and strengthened always by compression in that direction ; but in most cases the restored light is wanting in delicacy of outline, general distinctness, and steadiness—a clear proof that the molecular structure of the oil is more or less roughly disordered by the electric action. At 0° and at 90° the light is still restored by the electric action, and generally to almost as great intensity as in the case of 45°. Judging from all the observations, I think that the action of castor-oil is similar to that of olive-oil.

42. *Summary.*—Of the liquids examined, there are six which have given definite and constant results, namely these— carbon disulphide, benzol, paraffin and kerosene oils, oil of turpentine, olive-oil. These bodies are distinctly birefringent when dielectrified, acting upon transmitted light as uniaxal crystals with axes directed along the lines of force, the uniaxal being negative in the case of olive-oil, positive in the other five cases. Dielectrified olive-oil acts in the same way as dielectrified glass, or as glass compressed along the lines of force ; the other five liquids dielectrified act as resin dielectrified, or as glass extended along the lines of force. Compared among themselves with

reference to strength of birefringent action, the liquids appear to be very unequal—carbon disulphide the strongest, paraffin and kerosene the weakest. Compared with glass, they are much weaker insulators ; but if allowance be made for this difference, I think that, for intensity as well as purity of effects, carbon disulphide is far superior to glass. In contrast with glass, all the liquids are characterized by the absence of coercive force, and by the rapidity of variation of birefringent action from point to point of the electric field. The birefringent power is sustained in liquids by the present action of electric force at each instant ; it seems also to be determined at each point simply by direction and intensity of force at the point.

43. *Theory.*—After the explanations already advanced in the first paper (24), and again in this paper (34), I think I need do no more than enunciate the three following assumptions :—

1. The particles of dielectrified bodies tend to arrange themselves in files along the lines of force.

2. Changes of molecular arrangement consequent upon rise or fall of electric action are effected slowly and with difficulty in solids, easily and at once in liquids.

3. The lines of electric force, or the axes of molecular files, are lines of compression in one class of dielectrics (glass &c.), and lines of extension in another class (carbon disulphide &c.).

The facts, when thus interpreted, afford a strong confirmation of Faraday's theory of electrostatic induction ; and in whatever way interpreted, they give promise of some new insight into that interesting subject, the molecular mechanism of electric action.

I cannot conclude without expressing a hope, amounting almost to a belief, that the plate-cell charged with carbon disulphide will develop from the present rude beginning into a valuable physical instrument, a very delicate optical electrometer.

Glasgow, October 20, 1875.

LV. *On the General Theory of Duplex Telegraphy.*
By Louis Schwendler[*].

[Continued from vol. xlix. p. 126.]

IN the two preceding investigations[*] I have given the solution of the *first problem* for the bridge method. This solution established the general result of the *double balance* being the best possible arrangement for the bridge method. In the present paper I shall endeavour to find the solution of the

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[*] Journal of the Asiatic Society of Bengal, vol. xliii. part 2, 1874, pp. 1 and 218. Phil. Mag. 1874, vol. xlviii. p. 117; and 1875, vol. xlix. p. 108. *Journal Télégraphique,* vol. ii. p. 580.

first problem for the *differential method,* which in practical importance ranges second to the bridge method.

II. *Differential Method*[*].

This arrangement for duplex working is based on the well-known method of comparing electrical resistances " differential method ;" and fig. 2 gives the general diagram when this method is applied for duplex working.

Fig. 2.
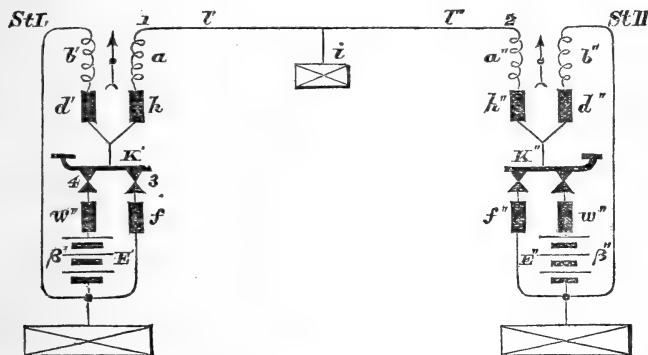
E, electromotive force of the signalling battery.

β, internal resistance of the signalling battery.

k, a constant-resistance key.

a and b, the coils of the receiving-instrument. These coils, for any sent current, have opposite magnetic effects with respect to any given magnetic pole external to the coils; while for any received current these coils add their effects with respect to that same magnetic pole. By a and b shall also be designated the resistances of the coils.

$d, w, f,$ and h are certain resistances, the necessity of which will become clear hereafter.

i, the resistance of the resultant fault of the line, acting at a distance l' from station I., and at a distance l'' from station II. (both l' and l'' expressed in resistances, so that $l'+l''=L$ equal the " real conduction-resistance " of the line).

The other terms, viz. L', L'', ρ', ρ'', c', c'', &c., which will necessarily be of frequent occurrence also in this paper, will bear the same physical meaning as before.

The practical inferiority of the *differential method,* when compared with the *bridge method,* it will be clear at once, is that specially constructed receiving-instruments on the differential

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[*] The differential method was originally invented, as stated before, by Mr. Frischen and by Messrs. Siemens and Halske. A particular case of this method was patented by them in England in 1854.

principle are required, and that, therefore, the introduction of duplex telegraphy based on the differential method would at once involve also a total change of the receiving-instruments hitherto used.   This is clearly a serious disadvantage from an administrative and financial point of view.   But, besides this, without going into details, the differential method has also a very serious objection from a technical point of view.   While in the bridge method the balance is obviously independent of the resistance of the receiving-instrument, in the differential method the balance is clearly a function of the resistances of the two coils of which the receiving-instrument consists; and as these two coils may alter their resistances independently, and not in proportion as indicated by the balance-equation, a new element of disturbance is introduced, which the bridge method certainly does not possess.

Besides this, differential instruments are necessarily mechanically more complicated than others, and require therefore superior workmanship, entailing greater expense to arrive at working efficiency.

General expressions for the two functions "D" and "S."

In order to obtain the two functions D and S, we have to develop the general expressions for p, P, and Q, say for station I.

p' in our particular case is the force exerted by the two coils a' and b' on one and the same magnetic pole when station I. is sending and station II. is at rest.   This force is clearly the difference of the two forces exerted by the coils a' and b'.

Thus we have

$$p' = A'm' - B'n',$$

where A' and B' are the currents which pass through the two coils a' and b' respectively when station I. is sending and station II. is at rest, while m' and n' are the forces exerted by these coils when the unit current passes through them.   At balance in station I.

$$p' = 0.$$

Further,

$$P' = \mathfrak{A}'m' + \mathfrak{B}'n',$$

where $\mathfrak{A}'$ and $\mathfrak{B}'$ are the currents which pass through the coils a' and b' respectively when station II. is sending and station I. is at rest (single signals).

Further,

$$Q' = V'm' + \mathfrak{q}'n',$$

where V' and $\mathfrak{q}'$ are the currents which pass through a' and b' respectively when both stations are sending simultaneously (duplex signals).

To get the most general expressions for these three forces, p,

P, and Q, we have to fix the signs of the two terms of which they consist. This is best done by considering the forces m and n as absolute numbers, and determining the direction in which they act with respect to one and the same magnetic pole by the direction of the currents passing through the coils a and b.

To fix the signs of the currents, we shall call, arbitrarily, that current positive which passes through the coil a in the sending-station when the negative pole of the signalling-battery is joined to earth.

Further, if we suppose at the outset that the movement of the key k does not alter the complex resistance ρ of its own station, *i. e.* the fulfilment of the key-equation

$$w + \beta = f,$$

a condition which is essential, it is clear that the currents $\mathfrak{y}'$ and $\mathfrak{q}_{\prime}$ are the algebraical sums of the currents A', $\mathfrak{A}'$ and B', $\mathfrak{B}'$ respectively; whence it follows that

$$Q' = (A' + \mathfrak{A}')m' + (B' + \mathfrak{B}')n',$$

where the currents contain the signs.

Now, with respect to the manner of connecting up the two signalling-batteries E' and E'', we have the following two different cases:—

1st. The same pole of the signalling-battery is connected to earth in each station, thus:

$$p' = \pm A'm' \mp B'n',$$
$$P' = \mp \mathfrak{A}'m' \mp \mathfrak{B}'n',$$
$$Q' = (\pm A' \mp \mathfrak{A}')m' + (\mp B' \mp \mathfrak{B}')n',$$

where the upper signs are to be used when the negative poles of the signalling-batteries are connected to earth in both stations, and the lower signs when the positive poles of the signalling-batteries are connected to earth in both stations.

2ndly. Opposite poles of the signalling-batteries are connected to earth in the two stations, thus:

$$p' = \pm A'm' \mp B'n',$$
$$P' = \pm \mathfrak{A}'m' \pm \mathfrak{B}'n',$$
$$Q' = (\pm A' \pm \mathfrak{A}')m' + (\mp B' \pm \mathfrak{B}')n',$$

where the upper signs are to be used when the negative pole in station I. and the positive pole in station II. are connected to earth, and the lower signs when the reverse is the case.

Subtracting in either of these two cases Q' from P', it will be seen that invariably

$$S' = Q' - P' = p';$$

or that, on account of having fulfilled the key-equation $w + \beta = f$,

the difference of force by which single and duplex signals are produced is equal in magnitude and sign to the force by which balance is disturbed; further, that it is perfectly immaterial whether the same or opposite poles of the signalling-batteries are put to earth. For reasons already explained, I prefer to use the negative poles of the signalling-batteries to earth in both stations; and this alternative we will suppose is adopted.

Thus we have

$$p' = A'm' - B'n',$$
$$P' = -(\mathfrak{A}'m' + \mathfrak{B}'u')$$
$$Q' = (A' - \mathfrak{A}')m' - (B' + \mathfrak{B}')n'.$$

If we now substitute for A', B', $\mathfrak{A}'$, $\mathfrak{B}'$ their values, we get

$$p' = \frac{E'}{N'}\Delta',$$
$$P' = -\frac{E''(b'' + d'')}{N''}\mu'\lambda',$$

and

$$Q' = -\frac{E''(b'' + d'')}{N''}\mu'\lambda' + \frac{E'}{N'}\Delta',$$

the sign of p' being contained in Δ', and where

$$N' = f'(b' + d' + a' + h' + c') + (b' + d')(a' + h' + c'),$$
$$N'' = f''(b'' + d'' + a'' + h'' + c'') + (b'' + d'')(a'' + h'' + c''),$$
$$\mu' = \frac{i}{i + l' + \rho'},$$
$$\Delta' = (b' + d')m' - (a' + h' + c')n',$$
$$\lambda' = m' + \frac{f'}{b' + d' + f'}n'.$$

Thus the general expressions for the two functions D and S are

$$\left.\begin{array}{l} D' = \dfrac{p'}{P'} = \dfrac{E'}{E''} \cdot \dfrac{N''}{N'(b'' + d'')} \cdot \dfrac{\Delta'}{\mu'\lambda'}, \\[2ex] S' = p' \quad = \dfrac{E'}{N'}\Delta', \end{array}\right\} \text{ for station I.,}$$

and

$$\left.\begin{array}{l} D'' = \dfrac{p''}{P'} = \dfrac{E''}{E'} \cdot \dfrac{N'}{N''(b' + d')} \cdot \dfrac{\Delta''}{\mu''\lambda''}, \\[2ex] S'' = p'' \quad = \dfrac{E''}{N''}\Delta'', \end{array}\right\} \text{ for station II.}$$

Rigid fulfilment of the two functions D $=0$ *and* S $=0$.

D can only become zero for finite resistances of the branches if

$$p = S = 0,$$

i. e. if

$$\Delta = 0.$$

Now, to keep $\Delta = 0$, we may adopt two essentially different modes of readjustment, namely :—

Either leave the two coils and their armatures stationary, and adjust balance by altering the resistances of the branches $(a + h)$ and $(b + d)$ separately or simultaneously, or leave the resistances of these branches constant and move the coils or their armatures. These two cases are to be considered separately.

(a) *Readjustment of balance by altering the resistances of the branches.*

As a and b are resistances which in the form of coils have to exert magnetic force, it is impracticable to suppose them variable. If they have been once selected, they must necessarily be kept constant, whence it follows that the readjustment of balance is restricted to a variation of one or both of the resistances h and d.

But as ρ is a function of h and d, to establish balance by altering one of them only would invariably result in an alteration of ρ, and consequently *immediate* balance would become an impossibility.

Thus in order to readjust balance, and at the same time to keep ρ constant*, we must vary h and d simultaneously.

Now it can be proved, in exactly the same manner for the differential method as was done for the bridge, that in order to make the disturbance of balance for any given variation in the system as small as possible we must make ρ as large as possible, whence it follows from the form of ρ that

$$f = b + d,$$

the "*regularity-condition*" for the differential method.

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* $\rho = a + h + \dfrac{(b+d)f}{b+d+f}$;

keep a, b, and f constant, and vary h and d; whence we should have

$$\delta\rho = (b+d+f)(b+d+f+\delta d)\delta h + f^2 \delta d = 0,$$

an equation which it is always possible to fulfil for any variations of h and d if taken of opposite signs, although it may be difficult to achieve it practically by a simple motion, such as that of turning a handle. The absolute value of these variations depends, of course, on the variation of c which disturbs the balance; and in order to have accelerated balance we ought to decrease h and increase d when c increases, and *vice versâ*.

But, since

$$f = w + \beta,$$

it follows that to reestablish balance by an alteration of the resistances h and d while a, b, β, and ρ keep constant, we have to vary *all* the four branches h, d, w, and f simultaneously in such a manner that their variations fulfil the following condition,

$$\delta f = \delta d = \delta w = -(2\delta h),$$

which is simple enough to allow of its practical application, but which nevertheless shows again the inferiority of the *differential method* as compared with the *double balance*; i. e., in order to fulfil *immediate balance*, the *key-equation*, and the *regularity-condition* for the *differential* method, we have to make the four branches of the system simultaneously variable, while in the *double balance* the same effect can be obtained by having only *one* branch variable (the b branch).

It is worth while to mention here that there is a special case of obtaining *immediate balance* for the differential method by the adjustment in one branch, namely when $f = 0$; for then ρ would be independent of d, and therefore balance could be obtained by varying d without altering ρ.

However, on account of the key-equation $f = w + \beta$, it would follow from $f = 0$ that β must be zero also; which represents a physical impossibility, inasmuch as the internal resistance of galvanic cells cannot be reduced to zero, not even approximately. Besides, the electromotive force requisite for duplex working being necessarily comparatively large, β will always be a quantity which cannot be neglected against the other resistances of the system, even if the single cells were of small resistance.

But supposing it were practicable to construct a battery of exceedingly low internal resistance, then, as $f = b + d$, it would be necessary to make $b = 0$ and $d = 0$, another physical impossibility, as b must consist of convolutions to produce magnetism, and d must be variable to produce balance.

This solution $f = b + d = w + \beta = 0$, or even each of these three branches of an only exceedingly small resistance, must therefore be rejected.

(b) *Adjustment of balance by moving the coils or armatures.*

This, it will be clear, is *the* solution for *immediate balance*; for such a mode of adjustment would involve no relation between the resistance of the three branches, leaving their determination free for other purposes. In order that the slightest movement of the two coils or their armatures may produce the required balance, it will be best to move both the coils or armatures simultaneously in the same direction. In fact, to be able to produce

balance no matter how great the variation in the resistance of the line may become, it will be necessary to make the coils movable for the changes of seasons, and the armatures for the daily changes.

It is clear that the differential method, when balance is adjusted by the movement of the coils or armatures, can alone be compared in efficiency with the *double balance*; and the superiority of the latter is most striking. While *immediate balance* and the fulfilment of the other two essential conditions can be obtained with *the double-balance method* within any given range by a variation of the resistance in *one* single branch (*b* branch), this same result with the differential method can only be arrived at by either supposing four branches simultaneously variable, or by supposing the coils and armatures movable—both presupposing complicated mechanical arrangements requiring delicate workmanship and being liable to get out of order.

Rapid approximation of the two functions D *and* S *towards zero.*

Supposing the fulfilment of the key-equation as one of the most essential conditions, we know that

$$p = S \text{ for each station invariably.}$$

Now for station I. we have

$$p' = S' = E' \frac{\Delta'}{N'},$$

where

$$\Delta' = (b' + d')m' - (a' + h' + c')n',$$
$$N' = f'(b' + d' + a' + h' + c') + (b' + d')(a' + h' + c').$$

If we call c' that value of the measured circuit which for any given values of the two branches $b' + d'$ and $a' + h'$ produces balance in station I. (*i. e.* for which $\Delta' = 0$), then, if c' varies $\delta c'$, we have $\Delta' = n'\delta c'$, while N' becomes $N' + \delta N'$.

Thus we have

$$S' = E' \frac{n'\delta c'}{N' + \delta N'},$$

$$S' = \frac{E'n'}{f' + b' + d'} \cdot \frac{\delta c'}{a' + h' + \dfrac{f'(b' + d')}{f' + b' + d'} + c' + \delta c'};$$

but as $a' + h' + \dfrac{f(b' + d')}{f' + b' + d'} = \rho'$ the complex resistance in station I., and as further $\delta c'$ can be neglected against c', we have finally

$$S' = E' \frac{n'}{f' + b' + d'} \cdot \frac{\delta c'}{c' + \rho'}.$$

Further, n', the force exerted by the coil b' on a given magnetic pole when the unit current passes through the coil, can be expressed as follows :—

$$n' = r'\sqrt{b'}*,$$

where r' is a coefficient depending only on the dimensions and shape of the coil, on the manner of coiling the wire, and on the integral distance of the coil from the magnetic pole acted upon.

Thus we have

$$S' = E'\frac{r'\sqrt{b'}}{b'+f'+d'} \cdot \frac{\delta c'}{c'+\rho'} = E'.W'.\theta'.$$

Now, supposing the factor W' constant†, S' becomes smaller the smaller θ is.

In the second part it has been proved quite generally that θ decreases permanently with increasing ρ' and ρ'', no matter to what special cause the variation of c' is due, whence again it follows that ρ should be a maximum.

From the form of ρ, however, we see that for any given sum $b+f+d$, ρ becomes largest if

$$f = b + d,$$

which is "*the regularity-condition*" of the differential method.

To have S, therefore, for any variation as small as possible we must make $f = b + d$. Substituting this value of f, we get an expression for S which shows that it has an absolute maximum for b, but no minimum, from which we conclude that b should be made either very much smaller or very much larger than the value which corresponds to a maximum of S ; but no fixed relation between b and d or a can be found.

In order to prove that $b + d = f$ is *the solution*, we must now how that it also makes D as small as possible.

But as

$$D = \frac{S}{P},$$

we have only to show that the regularity-condition $b + d = f$

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* This expression supposes that the thickness of the insulating covering of the wire can be neglected against the diameter of the wire, which is allowable. r' is a constant with respect to b'.

† That W' can be kept constant while θ' decreases and $\dfrac{f'}{b'+d'}$ varies and $f'+b'+d'$ is constant, it will be clear is possible; for if $d' > 0$, the variation of $b'+d'$ may be considered entirely due to a variation of d' equal and opposite in sign to the variation of f'. If $d = 0$, then we must consider r' variable with b' in order to keep W' constant while $\dfrac{f}{b}$ varies, which is admissible, since the position of the coils has not yet been fixed.

makes P either as large as possible or, which would be still better, a maximum.

Now
$$P' = A \; \mu' \lambda',$$

where A'' is the current which enters the line at point 2 (fig. 2) when station II. is sending alone, while μ' is the factor wh ch determines the loss through leakage of the line, and λ' is the factor to which the magnetic force exerted by the current $A'' \mu'$ in station I. is proportional.

μ' and λ' are functions of the resistances in station I. only [*], but not of those in station II.

Now, for constant values of μ' and λ' (*i. e.* leaving every thing in station I. constant), P' becomes larger the larger A'' is.

$$A'' = E'' \frac{b'' + d''}{N''}.$$

Substituting its value for N'', and dividing numerator and denominator by $b'' + d''$, we get

$$A'' = \cfrac{E''}{f'' + \cfrac{f''(a'' + h'')}{b'' + d''} + a'' + h'' + c''\left(1 + \cfrac{f''}{b'' + d'}\right)}.$$

Supposing balance in station II. rigidly fulfilled, we have

$$(b'' + d'')m'' - (a'' + h'' + c'')n'' = 0;$$

$$\therefore \quad c'' = (b'' + d'')\frac{m''}{n''} - (a'' + h'').$$

Substituting this value of c'' in the expression for A'' and reducing, we get

$$A'' = \frac{E''r''\sqrt{b''}}{f''r''\sqrt{b''} + q''(b'' + d'' + f'')\sqrt{a''}}.$$

Dividing by q'' and putting $\dfrac{r''}{q''} = v''$, we have

$$A'' = E'' \frac{v'\sqrt{b''}}{f''v'\sqrt{b''} + (b'' + d'' + f'')\sqrt{a''}}.$$

This expression has a maximum[†] for

$$b'' = f'' + d'',$$

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[*] $\mu' = \dfrac{i}{i + l' + \rho'}$; $\lambda' = m' + \dfrac{f'}{f' + b' + d'}n'.$

[†] In order to keep the balance in station II. rigid when b'' varies, we must suppose v'' simultaneously variable with b''. This is perfectly justified; for v'' can be altered by an appropriate movement of the coils to keep up the balance in station II. without altering the outgoing current A''.

which contradicts the regularity-condition $f = b + d$ so long as d is different from zero.

Thus in order to fulfil the regularity-condition and the maximum current for the differential method simultaneously, we must put
$$d = 0.$$

However, it has been shown that in order to have immediate balance, when adjusting balance by a variation in the resistances, we have to alter the resistances of the four branches $b + d$, $a + h$, f, and $w + \beta$ simultaneously according to a relation already given. Thus it is proved that adjustment of balance by an alteration of the resistances must be rejected, since, as pointed out before, a variation of the resistances of the coil b is impracticable.

We are obliged therefore to adjust balance by moving the coils or their armatures ; and the further solution of the problem is only required when this mode of adjustment is adopted.

Maximum Magnetic Moment.

It has now been proved that d is to be made zero in order to be able to fulfil the conditions of *regularity* and *maximum current* simultaneously, and that therefore, to obtain immediate balance, readjustment of balance is to be effected by a movement of the two coils a and b or their armatures, and *not*, as has been generally proposed, by an alteration of the resistance in the branches $(a + h$ and $b + d)$.

Hence h appearing in the denominator of P only, and $h > 0$ not being any more required for adjusting balance, the best value we can give to h is
$$h = 0,$$
which will make P obviously largest[*].

Substituting therefore in the expression for P
$$h = d = 0,$$
$$f = w + \beta = b,$$
we get

$$P' = \frac{E''}{2(a'' + c'') + b''}\, \mu'\lambda' \text{ for station I.,}$$

and

$$P'' = \frac{E'}{2(a' + c') + b'}\, \mu''\lambda'' \text{ for station II.}$$

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[*] The resistances d and h, without exerting magnetic force, were originally introduced in order to investigate the possibility of adjusting balance by an alteration of the resistances in the branches.   But since it has been shown that this mode of adjustment is to be rejected, it is of course clear that the dead resistances in these branches should be made zero, when P will become largest.

These two expressions do not as yet contain the balance-conditions.

The factors

$$\frac{\mu'}{2(a''+c'')+b''} \text{ and } \frac{\mu''}{2(a'+c')+b'}$$

are identical, namely

$$\frac{\mu'}{2(a''+c'')+b''} = \frac{\mu''}{2(a'+c')+b'} = \frac{i}{Q},$$

where

$$Q = i\{2(a'+a''+l'+l'') + b'+b''\} + \frac{b'b''}{2}$$
$$+ (a''+l'')(a'+l'+b') + (a'+l')(a''+l''+b''),$$

as can be easily calculated by substituting for λ, a, and c their known values.

In the second investigation it has been stated why P′ and P″ cannot be made maxima separately, and that we could do nothing else but make their sum a maximum. In this case we have to do the same. Hence the question to be solved is reduced to the following :

$$P = P' + P'' = i \cdot \frac{E''\lambda' + E'\lambda''}{Q}$$

is to be made a maximum with respect to the variables a, b, q, and r, while they are linked together by two condition-equations, namely

$$r'(a'+c') - q'\sqrt{a'b'} = 0 \text{ balance in station I.,}$$

and

$$r''(a''+c'') - g''\sqrt{a''b''} = 0 \quad \text{,,} \qquad \text{,,} \qquad \text{II.}$$

This general problem can be solved in exactly the same way as it was in the second investigation. However, it is not needed to do this again, since the general solution can be written down from inference, after having solved the special problem for a line which is perfect in insulation.

Suppose that $i = \infty$, or at least very large as compared with $l' + l'' = L$, then obviously P′ and P″ become identical without condition ; namely,

$$P' = P'' = P = \frac{E}{4} \cdot \frac{2q\sqrt{a} + r\sqrt{b}}{L + 2a + b};$$

while the two balance-equations become also identical, namely

$$2q\sqrt{ab} - r(4a+b+2L) = 0.$$

If we substitute the value of r from the balance-equation in the expression for P, we get

$$P = Eq \cdot \frac{\sqrt{a}}{4a + 2L + b},$$

which has an absolute maximum with respect to a only; namely

$$a = \frac{L}{2} + \frac{b}{4}.$$

Substituting this value of a in the last expression for P, we get

$$P = \frac{Eq}{4} \cdot \frac{1}{\sqrt{2L + b}}.$$

Whence it follows that P becomes largest for $b = 0$; otherwise b remains indeterminate. q, on the other hand, should be made as large as possible.

If we now put $v = \frac{r}{q}$, and develop its value from the balance-equation, we get

$$v = \frac{r}{q} = \frac{1}{2} \sqrt{\frac{b}{2L + b}}.$$

The solution of the first problem of the differential method, when the line is perfect in insulation, is therefore

$$h = d = 0,$$
$$f = b = w + \beta,$$
$$a = \frac{L}{2} + \frac{b}{4},$$
$$v = \frac{1}{2} \sqrt{\frac{b}{2L + b}}.$$

The absolute value of b is left indeterminate*, and we only know that the smaller it can be made the better.

But to fulfil this best condition, $f = b = w + \beta = 0$ represents a physical impossibility, since neither β, the internal resistance of constant galvanic cells, can be made zero, not even approximately, nor b, which must have convolutions in order to act magnetically.

The larger $f = b = w + \beta$ becomes, for practical reasons, the more the differential method, even under the best quantitative arrangements as given above, will become inefficient as compared with the double balance.

Now by inference we get for a line with leakage, i. e. $i < \infty$,

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* Practically, however, it may be said that b is given; for generally β, the internal resistance of the signalling-battery, is determined by the nature and number of galvanic cells required for duplex working. We must only remember that b should be made somewhat larger than β, in order to have an adjustable resistance w in the battery branch, which may be used for compensating any variation of the battery-resistance, that the equation $f = b = w + \beta$ may be permanently fulfilled.

$$a = \frac{\mathrm{L}'}{2} + \frac{b'}{4},$$

$$a'' = \frac{\mathrm{L}''}{2} + \frac{b''}{4},$$

$$v' = \frac{1}{2}\sqrt{\frac{b'}{2\mathrm{L}' + b'}},$$

$$v'' = \frac{1}{2}\sqrt{\frac{b''}{2\mathrm{L}'' + b''}},$$

approximately.

The above values for a and v are somewhat too large; but in practical application they are quite accurate enough.

The physical reason that this solution for the differential method gives an indeterminate result, is simply due to the fact that the force which produces the signals in the differential method is due to the combined magnetic actions of *two* separate coils through which unequal currents pass, instead of to *one* coil as in the bridge method. On account of $b = f$, it follows that the current which passes through the b coil is only half of that passing through the a coil. Thus, in order to make the most of the arrived currents, b and f should be both equal to zero, or, in other words, placing *all* the convolutions in a and *none* in b must clearly give the greatest magnetic force. Obviously, however, such a solution could not fulfil the balance-condition in the sending-station.

The value of b should be chosen as small as practicable; and its minimum value is β, the internal resistance of the signalling battery. How much larger b should be taken depends on the absolute variation of β, *i. e.* on the constancy of the signalling battery. If the battery is very constant with respect to internal resistance, then b need be only very little larger than β, which determines the adjustable resistance w.

For instance, minotto cells can be easily prepared with an internal resistance of 10 B.A.U. per single cell. Their minimum resistance, obtained by working, is never less than 5 B.A.U.; and if the zincs are changed from time to time, their maximum resistance will scarcely ever be higher than 10 B.A.U.

Hence to make b about 50 per cent. larger than β will suffice, by which, if β is known, the greatest value of w is fixed.

The absolute value of β can be determined from the number of cells which have to be connected up successively, in order to work a given instrument through a given line, when the circuit fig. 2 is adopted. This absolute value of β will therefore not only depend on the electrical state of the line and the nature of the cells, but also on the absolute sensitiveness of the differential instrument employed.

To make β, therefore, as small as possible, a sensitive construction of the differential instrument becomes requisite; further, cells of high electromotive force and low constant resistance are best adapted for forming the signalling-battery. In order to get the widest limits in the variation of w, it is clear that *that* β should be selected which is calculated from the maximum number of cells required to produce the signals with sufficient force. The greatest number of cells is obviously required when the line is at its lowest insulation in India, during the monsoon.

The value $v = \dfrac{r}{q}$ is what has been termed the mechanical arrangement of the differential instrument*.

If $b = w + \beta$ has been determined by fixing β, then v has its smallest value for L largest, which is the case when the line is perfect in insulation—when the coil a must be closest to the magnetic pole acted upon, and the coil b furthest away from it.

The highest value of v we obtain by substituting the lowest L, *i. e.* when the line is at its lowest insulation—when the coil b must be nearest to the magnetic point acted upon, and the coil a furthest away from it.

Hence, the two limits of v being fixed by the known limits between which L varies, the extent of movement of the two coils is also fixed; and consequently, if q is chosen arbitrarily, the construction of the differential instrument is determined. But even q is not quite arbitrary, since we know the form, dimensions, and resistance of the coils which, for instance in Siemens's polarized relays on any given line, have to produce the magnetism in single circuit to get the signals with engineering safety.

The solution of the first problem of the differential method is therefore :—

1. *Balance in each station must be obtained by a movement of the two acting coils or their armatures, either singly or* (*better*) *simultaneously in the same direction, and* not *by an alteration of the resistances in the branches.*

2. If this mode of adjusting balance be adopted, then the solution is

$$d = h = 0,$$
$$f = b = w + \beta,$$
$$a = \frac{L}{2} + \frac{b}{4}.$$
$$v = \frac{r}{q} = \frac{1}{2}\sqrt{\frac{b}{2L + b}}.$$

* Journal of the Asiatic Society of Bengal, vol. xli. part 2, p. 148; Phil. Mag. vol. xliv. p. 166.

It will now be clear that the given solution fulfils the following essential conditions :—

(i) *Any variation of the resistance in the total system has the least possible disturbing effect on the receiving-instrument.*

(ii) *Any disturbance of balance can be eliminated by an appropriate movement of the two acting coils or their armatures, without disturbing balance in the distant station.*

(iii) *Conditional maximum magnetic moment of the receiving-instrument.*

(iv) *Conditional maximum current.*

Addendum.

Here I wish to give some additional explanations and corrections with reference to the first and second parts of this investigation.

Journal of the Asiatic Society of Bengal, vol. xliii. part 2, 1874.

Page 12.   I have substituted

$$c' = L' + \rho''$$

without stating that this expression for c' is only approximately true.   The correct expression for c' is clearly

$$c' = l' + \frac{i(l'' + \rho'')}{i + l'' + \rho''} *,$$

which approximates closely towards $L' + \rho''$ if $l'' + \rho''$ is sufficiently small as compared with i.   This for any line in good electrical condition will always be the case.

Page 9. In the footnote (Phil. Mag. August 1874, p. 125, line 7 of note), for "*as nearly as possible equal*" read "*as nearly as possible proportional.*"

Page 20 (Ibid. p. 137).

$$\frac{dG}{dg} = L(a^2 - g^2) + 2ag(d - g) = 0,$$

should be

$$\frac{dG}{dg} = L(a^2 - g^2) + 2a(ad - g^2) = 0.$$

Pages 19 and 224 (Phil. Mag. August 1874, p. 125, and Feb. 1875, p. 115), after having shown that

$$a + f = g + d,$$

I conclude at once that on account of equation (VI) $(ad - gf = 0)$,

$$a = g = d = f, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(VIII.)}$$

while mathematically it follows only that

$$a = g \text{ and } d = f.$$

* Phil. Mag. February 1875, p. 113.

These two equalities certainly do not contradict equation (VIII.); but they do not necessitate it.

The additional reason why equation (VIII.) should be chosen follows from the balance condition

$$ad - bc = 0,$$
$$\therefore \; b = \frac{ad}{c}.$$

Therefore b becomes largest for any given c and any given $(a + d)$ if we put $a = d$.

But b largest is required for two separate reasons:—

1. If the immediate balance is disturbed by an alteration of the resistance of one or more of the four branches, which may happen especially by f (*i. e.* β, battery resistance) varying, then ρ becomes at once a function of b, i. e. an increasing one with b. Thus in order to keep ρ as large as possible and at the same time as constant as possible, b should be selected largest.

2. Further, by making b as large as the circumstances will admit, we clearly have the largest sent and largest received currents—which will be clear without calculation. In fact later on (page 232 *) it has been shown that $a = d$ is the condition for the maximum signalling-current.

Note.—Since the 3rd of February, 1875, the main line from Bombay to Madras has been successfully worked duplex by means of the " double-balance method."

This line is worked direct, *i. e.* without any translating instruments, and is 797 miles in length; it consists almost throughout of No. $5\frac{1}{2}$ wire, B. W. G. (diameter $5\frac{1}{4}$ millims.), and is supported chiefly on the Prussian insulator.

The section of this line from Bombay to Callian is exposed to the destructive influence of a tropical sea climate. Between Callian and Poona the line passes over the Western Ghauts; the dense fogs during the cold weather and the heavy rains during the south-west monsoon on these hills seriously affect its insulation. From Poona to Sholapore and Bellary the line runs inland, and experiences a climate on the whole favourable for the maintenance of constant and high insulation. Between Bellary and Madras, however, the line again comes under the influence of a most unfavourable climate, especially just before and during the continuance of the north-east monsoon, when the atmosphere at a high temperature is saturated with moisture and salt, leaving conducting deposits on the surface of the insulators.

Consequently during the south-west monsoon the resultant fault is near Bombay; during the hot weather it shifts towards the middle of the line; and in November, when the rains set in at Madras and the weather on the Bombay side is clearing up, the resultant fault is situated close to Madras.

* Phil. Mag. S. 4. vol. xlix. p. 124.

By February next duplex working will therefore have been submitted to a most severe test, applied as it will have been for a whole year to a long line the electrical condition of which is highly variable with respect to season and locality; and its practicability will doubtless again be clearly proved, as has already been the case on the Calcutta-Bombay line (1600 miles), where, under no more favourable climatic conditions, duplex has for the past twelve months not only fulfilled but surpassed the expectations formed of t. No difficulties have been experienced, and, it is believed, never will be.

Strange as it may appear from a theoretical point of view, it will nevertheless be found in practice that a line worked duplex carries more than double the traffic of the same line worked singly; for it represents two lines carried on different posts far distant from one another, instead of two parallel lines on the same posts; and consequently the highly injurious effects of voltaic induction are eliminated.

Further, the receiving signallers, not being provided with keys, are unable to interfere with messages during their transmission.

Corrections and repetitions do not necessitate a stoppage of work; for they are obtained in the following manner :—the receiving signaller marks with a cross or underlines the words to be repeated, and places the message by the side of the sending signaller, who calls for the repetitions directly he has finished the message he is transmitting; and during this call the distant station may either send fresh messages or may also call for repetitions. Consequently single working need never be resorted to, and the simultaneous exchange of messages and corrections becomes continuous.

The Indian system of receiving (the sounder system which has now been universally recognized as the only right one for handsignalling) thus necessitates constant attention on the part of the receiving signallers; for any inattention on their part at once becomes known to the controlling officer.

LVI. *On the Flow of Electricity in a uniform plane conducting Surface.*—Part II.* *By* Professor G. CAREY FOSTER, *F.R.S.,* *and* OLIVER J. LODGE, *B.Sc.*†

43. IN an unlimited uniform conducting surface, at given points of which there are sources and sinks of electricity, the electrical condition of each point within a limited area must be determined by the electrical condition of its boundaries.

Moreover, if an equal similar area be taken in a *limited* conducting sheet, whose conductivity and thickness outside this area vary in any manner, the electrical state within the area will still be the same as in the previous case, if only the conditions at the boundaries remain unchanged; for evidently the electrical flow within the area will depend on the actual electrical state of the boundaries, and not upon the way in which this state has been produced.   Consequently the truth or falsehood of conclusions arrived at, like those given in Part I. of this communication, on the assumption of an unlimited uniform conducting surface, may be tested by experiments on a conducting sheet of finite size, if we can ensure that, at the edges, the electrical conditions are the same as they would be at the boundaries of a portion of the same size and shape taken in an infinite sheet in which there is a known distribution of sources and sinks.

44. In order to carry out such experiments, it is needful that the boundaries of the part of the conducting sheet examined should be formed either wholly by equipotential lines, or partly by equipotential lines and partly by lines of flow.   It is evident that an area within which there is a flow of electricity (unless the circuit is complete within this area, as in a conducting plate moving in a magnetic field) cannot be bounded entirely by lines of flow; for in that case no electricity could enter or leave the area.

Any line in a conducting sheet can be practically made to be an equipotential line by laying along it, and in close contact with the sheet, a conductor whose resistance in directions parallel to the sheet is insensible—for instance, a thick band of copper. And any line along which the sheet is bounded by non-conductors (*e. g.* the free edges of the sheet) is necessarily a line of flow, since no electricity can pass across such a boundary in either direction. The experimental conditions can thus be chosen so that a conducting area may be bounded either by equipotential lines wholly, or partly by equipotential lines and partly by lines of flow, placed so as to correspond to any given system of poles.

45. Methods of testing experimentally the conclusions deduced from theory, founded on the general principles above indicated, have been employed by various investigators.   Thus Kirchhoff, in his paper of 1845 (Pogg. *Ann.* vol. lxiv.), traced the form and distribution of the equipotential lines on thin sheets of metal bounded by four circular arcs, two of which, being free edges, were lines of flow and belonged to the same circle; while the others, of much smaller extent than the first two, were equipotential lines determined by the contact of small cylindrical electrodes (conducting wires) with the margin of the sheet.   He traced the course of the equipotential lines by fixing a wire connected with one terminal of a galvanometer in succession at

several points arbitrarily chosen, and in each case finding, by trial with a wire from the other terminal, a sufficient number of points having the same potential as the selected point. To ascertain the distribution required to make the difference of potential between consecutive lines constant, he determined a number of points along the straight line joining the electrodes, such that the difference of potential between each one and the next was just sufficient to compensate the constant electromotive force of a thermoelectric element. The form of the equipotential lines in some rather more complicated cases was determined in the same manner by Quincke*, and in other cases again by students in the Edinburgh University Laboratory†. Schwedoff‡ also has investigated the direction of the flow at various points in a rectangular sheet of tinfoil.

Recently an elaborate series of experiments, by a method similar to Kirchhoff's, has been carried out by Professor W. G. Adams§, who has also extended the same method of inquiry to conductors of three dimensions by the employment of conducting liquids (mercury, sulphate of copper, sulphate of zinc).

Verifications by Kirchhoff and by Mach of the theoretical expressions for the strength of the current at different parts of a conducting sheet have already been referred to (see Part I. § 15, footnote, Phil. Mag. S. 4. vol. xlix. p. 395).

Experimental measurements of resistance, in addition to the attempts of Kirchhoff already mentioned (Part I. § 1), have been made by Gaugain‖, who measured the resistance of a solution of sulphate of copper contained between two circular copper cylinders placed one inside the other with their axes parallel, and thus verified Blavier's formula (see Part I. § 24, footnote)—by v. Obermayr¶, who verified Kirchhoff's formula for circular disks, and also a formula deduced by Stefan for the resistance of an oblong strip with circular electrodes placed on the central line, by experiments with disks and strips of platinum-foil—and by Domalip**, who measured the resistance of thin circular layers of a solution of sulphate of zinc contained between parallel glass

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* Pogg. *Ann.* (1856) vol. xcvii. p. 382.

† W. Robertson Smith, Proc. Edinb. Roy. Soc. 1869–70, p. 96.

‡ Schwedoff (Pogg. *Ann.* 1873, Ergänzungsb. vi. p. 85) inferred the direction of the resultant flow at any point of a conducting sheet in which there are two equal opposite poles, from simple geometrical considerations, to a great extent similar to those employed by Professor W. R. Smith and by ourselves (Phil. Mag. vol. xlix. p. 394). We should have referred to his paper before, if we had known of its existence.

§ Proc. Roy. Soc. (1875) vol. xxiii. p. 280.

‖ *Ann. de Chim. et de Phys.* [3] vol. lxiv. p. 200 (1862).

¶ *Wien. Akad. Ber.* (1869) vol. lx. pt. 2, p. 245.

** Carl's *Repertorium für Experimental-Physik*, 1874, vol. x. p. 23.

disks, the electrodes being zinc rods inserted through holes in the upper piece of glass.

46. The results of all these investigations are in such close agreement with calculation that it cannot be considered that any further confirmation of the general theory is needed. It nevertheless seems worth while to point out, and to illustrate by specimens of the results obtained by it, a simple method which serves both for determining the form and distribution of the equipotential lines on a conductor of two dimensions, and for measuring the resistance of the part of it contained between any two given lines. In all except the very simplest cases, it would be much easier to determine the equipotential lines and resistance experimentally in this way than by the corresponding calculation.

47. The method referred to is an application of the principle of Wheatstone's bridge, and will be intelligible from fig. 1. The poles of a battery (Z) are connected with the two ends of a graduated German-silver wire, E F, and also with two fixed points, A and B, of the conducting disk on which the equipotential lines are to be traced ; a galvanometer, G, is connected to a sliding contact block D on the wire E F, and to a contact-pin C, with the point of which the sheet can be explored. Under these circumstances the points C and D will be at the same potential whenever the galvanometer shows no deflection ; hence if the pin C be moved about on the disk, and a mark be made at every position for which the galvanometer is not affected, all these marks will lie on one equipotential line. By repeating this operation, after shifting the slider D along the graduated wire, another equipotential line can be marked out on the sheet ; and any number of lines can be traced, so as to correspond to equal differences of potential, by shifting the slider through successive equal distances.

Fig. 1.

48. In the actual experiments the connexions were not made exactly as shown in fig. 1, but the positions of battery and galvanometer were interchanged. This arrangement made it unnecessary to use a separate key for closing the battery-circuit, as the circuit was completed by the contact of the pin C with the sheet ; and since these contacts were only momentary, no sensible disturbance can have arisen from unequal heating. The
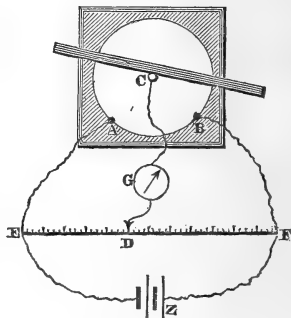
battery used was a single cell of Smee; the galvanometer was a reflecting one of low resistance. The disk was a circular piece of tinfoil, of 27·8 centim. diameter, pasted on the bottom of a tray or shallow box of hard wood, on the edges of which rested a wooden bar supporting the contact-pin C. This last slid in a vertical tube fixed to the wooden bar; a spiral spring inside the tube kept the pin out of contact with the sheet except when it was pressed down. When the right position on the sheet had been found by means of the blunt point of the pin, this position was marked by pressing the pin down harder, so as to make a slight indentation in the tinfoil.

The mode of making contact at A and B which we found to answer best is shown in fig. 2. A S is a brass spring with a projecting pin A, and a screw S with nut and washer. The screw passes through the wood of the tray and is fixed by screw-ing up the nut, the conducting wire being clamped between

Fig. 2.
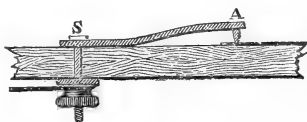
the nut and washer. The rounded end of the pin A rests on the tinfoil surface, pressing it firmly and making a good circular contact. It was found necessary to lubricate the bar carrying C with blacklead to prevent its causing vibrations when sliding on the edges of the tray, and so deranging the contact of the springs A and B.

49. After a series of points have been marked on the tinfoil in the manner described, a line may be drawn through each set by hand. A set of lines obtained in this way for the case of two equal opposite poles on the edge of a circular disk correspond to a portion of the diagram in Plate IX. (Part I.) bounded by a complete line of flow. P being any one of the dots marking such a line, if they are circles with the poles as inverse points, the ratio $\frac{AP}{BP}$ will be constant for any one line (Part I. § 16); and if they are at equal differences of potential, the values of this ratio will vary from one line to the next by a constant factor μ (Part I. § 18). The numbers given in the following Table show to what extent these requirements are fulfilled by the ex-perimental lines. Nine or ten points on an average were deter-mined for each line; but as a specimen of the results it seems sufficient to give the ratios found for four points only, viz. (in the case of each line) for a point near either end, a point on or near the straight line joining the poles, and a point about halfway between this and the more distant end of the equipotential line.

Values of $\dfrac{\text{AP}}{\text{BP}} (=\mu^n)$ *for for four different points on each of*

eight different equipotential lines.

	μ^{-2}.	μ^{-2}.	μ^{-1}.	μ^{0}.	μ.	μ^{2}.	μ^{3}.	μ^{4}.
	·200	·335	·568	·996	1·71	2·94	5·05	8·75
	·200	·327	·568	1·00	1·70	2·95	5·13	9·01
	·190	·323	·573	1·00	1·71	2·98	5·18	8·97
	·196	·333	·573	1·02	1·73	2·98	5·53	8·91
Means.	·1965	·3295	·5705	1·005	1·71	2·96	5·22	8·66
	$=(1·72)^{-3}$	$=(1·74)^{-2}$	$=(1·76)^{-1}$		$=(1·71)$	$=(1·72)^2$	$=(1·73)^3$	$=(1·71)^4$

In the experiments by which the above values were obtained the distance between the poles was 19·45 centims. Taking this as the value of $2a$, and 1·724 for μ, a system of coaxal circles was drawn by the method of § 21, Part I. A tracing of the experimental curves was then superposed on these theoretical circles; and the two sets were found to coincide almost exactly.

Another case tried was that of a circular disk with four equal poles on its edge, two positive and two negative; the equipotential lines obtained experimentally agreed in all particulars with those drawn by the superposition method of § 35.

50. The arrangement of fig. 1 serves not only to lay out the equipotential lines on a disk, but also to determine its resistance; for if the pin C be left in contact with any point of the disk (such as the electrode A for instance), while D is adjusted so that the galvanometer stands at zero, and if C is then moved to another point (say the electrode B) and D again adjusted, the resistance of the part of the disk between the equipotential lines which respectively pass through the two positions of C is the same fraction of the resistance of the branch E A B F, as the resistance of the part of the wire between the two positions of D is of the resistance of the whole wire E F. Accordingly the resistance of the disk is given, in terms of that of unit length of the wire E F, as soon as the ratio of the resistances E A B F and E D F is known. In order to determine this ratio, the connexions shown in fig. 1 were somewhat modified, the arrangement finally adopted being that shown in fig. 3.

One pole of the battery is connected by means of a slider D with some point of the wire E F, while the other can be connected at pleasure, on the one hand, with either of the electrodes A and B upon the disk by pressing one of the contact-keys 1 and 2, or, on the other hand, with another slider H on the wire E F by pressing the key 3. The galvanometer is permanently connected with one end E of the wire, and with a fixed point K

between the sliders D and H. To measure the resistance between A and B in terms of that of the wire E F, it is then only neces-

Fig. 3.
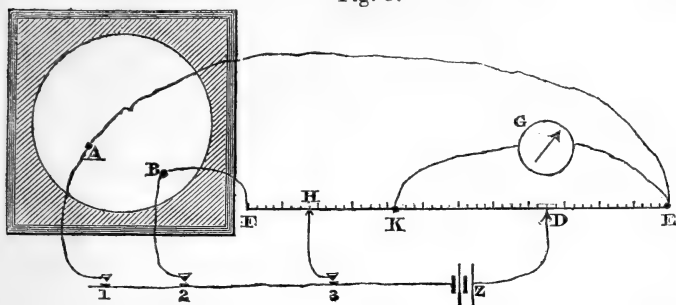
sary to press successively the keys 1 and 2, and each time to adjust the slider D so as to give no deflection on the galvano-meter, and then to connect the battery with H, by the key 3, and to find the positions of D required to keep the galvanometer at zero for two positions of H separated by the known amount h. If we denote by s the distance through which D has to be shifted on the wire E F when the battery-contact on the disk is trans-ferred from A to B, and by d the shift of D corresponding to the shift of H over the distance h, we have

$$\frac{\text{resistance between A and B}}{\text{resistance of } s} = \frac{\text{resistance of } h}{\text{resistance of } d};$$

and since s, h, and d are all of them lengths measured on the same wire, their resistances are simply as their lengths. Hence, taking as unit of resistance the resistance of unit length of the wire E F, we have
$$\text{resistance A to B} = s\frac{h}{d}.$$

It will be observed that the ratio $\frac{h}{d}$, which we will hereafter de-note by $\frac{1}{\epsilon}$, is the same as that of the resistances E A B F and E D F.

51. In all measurements of resistance it is necessary, not only that the contact of the electrodes A and B with the disk should be constant and easily reproduced, but also that the area of con-tact should be measurable; the difficulty of realizing these conditions with accuracy is by far the most important source of error to which the determinations of resistance are liable. In order to obtain a measurable and constant surface of contact, the spring contact-pieces employed in the experiments for plotting

the equipotential lines were replaced by the following arrangement. A thin cylindrical rod of brass, to which two wires could be attached, slid vertically in a V-groove down the side of a rectangular wooden block. The lower end of the rod was filed accurately flat and perpendicular to its length, so as to touch the tinfoil with its entire surface. Constant pressure against the tinfoil was maintained by placing a weight on a cap fixed to its upper end. Two such rods constituted the electrodes A and B; and tolerably constant contacts could be made with them after a little practice. Nevertheless individual readings are apt to vary a good deal, and it is necessary to take the mean of several distinct experiments in order to get a single trustworthy result.

The numbers given in the annexed Table will serve to illustrate the variations occurring among single readings. They refer to a set of experiments in which the electrodes were placed approximately at the ends of a diameter of a circular disk.

	$2a$.	s.
	26·30	69·4
	26·22	69·7
	26·30	68·3
	26·30	68·3
	26·00	67·5
	26·31	68·7
	25·95	69·0
	26·25	68·6
	26·30	69·8
	26·00	69·4
	26·25	67·9
	26·31	68·7
	26·05	69·2
Means ......26·20		68·80

52. The principal results of the measurements of resistance made by the method explained above are given in the Tables which follow, in such a way as to show how far they agree with the values indicated by theory. Every entry in the Tables is a mean value deduced from the number of experiments stated in the first column; the last column in each Table contains a quotient which theoretically should be constant. Common logarithms are employed, except when it is otherwise stated. The symbols used in the headings of the columns have the following significations:—

R, resistance in terms of an arbitrary unit ($= \cdot 00155$ ohm; being the resistance of one centimetre of the wire E F).

x, resistance of the contacts between the electrodes and the sheet (found by special experiments).

e, ratio of the resistances E D F and E A B F (figs. 1 or 3), or $\dfrac{d}{h}$ (§ 50).

s, shift of the slider D.

$2a$, or AB, distance between the poles.

ρ, radius of the circle of contact of the electrodes, or half the diameter of the brass rods.

r, radius of the tinfoil disk.

δ, thickness of the disk.

κ, specific conductivity of the material of disk.

L, the expression $\log_{10}\dfrac{\text{AG . BF . A'G . B'F}}{\rho^2 . \text{A'F . B'G}}$ (see equation $(20a)$, § 39, Part I.).

C, the constant $\dfrac{\log_e 10}{2\pi\kappa\delta}$.

The following relations hold good among the above quantities:

$$\text{R} + x = \frac{s}{\epsilon} \quad (\S\ 50)$$

and

$$\text{R} = \text{CL} \quad (\S\S\ 38 \text{ and } 39, \text{ Part I.}),$$

whence

$$s - \epsilon x = \epsilon\text{CL}.$$

First set of Resistance experiments, April 1874.

ϵ was practically constant and $= 5\cdot147$, $\rho = \cdot163$, $\epsilon x = 2\cdot42$.

Case 1. Two equal electrodes on the edge of a circular disk (Part I. Plate IX.).

Number of experiments.	r.	$2a$.	s.	$\dfrac{s - \epsilon x}{\log\dfrac{2a}{\rho}}$.
13		26·20	68·80	30·1
13		18·83	65·42	30·5
9	13·16	15·465	62·08	30·0
12		13·466	59·47	29·8
16		10·185	57·05	30·6
17		15·224	61·66	30·1
6		11·675	58·02	30·0
10	7·62	10·35	57·20	30·4
5		8·78	56·60	31·3
		Weighted mean......		...30·29

In this case $\text{L} = 4\log\dfrac{2a}{\rho}$ by formula (13), §§ 25 & **39, Part I.,**

and therefore $\dfrac{s - \epsilon x}{\log\dfrac{2a}{\rho}} = 4\text{C}\epsilon = \text{const.}$: the mean value 30·29 gives

$2\text{C} = 2\cdot943$.

Case 2. Two equal electrodes, one at the centre and one on the edge of a circular disk.

Number of experiments.	r.	s.	$\dfrac{s-\epsilon x}{\log\dfrac{r}{\rho}}$.
8	13·16	46·47	23·10
7	7·62	41·81	23·60
		Mean ......23·33	

Here $L = 3\log\dfrac{r}{\rho}$ by formula (21), § 39, Part I., and there-fore $\dfrac{s-\epsilon x}{\log\dfrac{r}{\rho}} = 3C\epsilon = 23\cdot33$, whence $2C = 3\cdot022$.

The lines of flow and of equal potential for this case are shown in fig. 6, Plate X. Part I., by the part of the figure inside the circle. This figure represents the case of two opposite poles in an unlimited sheet, one of them being twice as strong as the other. One of the flow-lines is then a circle passing through the pole of double strength and having the other pole at its centre. The remaining flow-lines lie half of them within this circle and half of them outside. Consequently, if the sheet be cut along the circumference of this circle (which in the figure is shown as complete), a disk is obtained in which the electrical conditions are the same as in this experiment.

Case 3. Both electrodes in the disk, one on each side the centre and at equal distances from it.

Number of experiments.	r.	AB.	s.	$\dfrac{s-\epsilon x}{\log\dfrac{5\,.\,\text{AB}}{3\,.\,\rho}}$.
4	13·16	13·16	36·92	16·20
5	7·62	7·62	32·78	16·05
			Mean ......16·12	

Here L becomes $2\log\dfrac{5\,.\,\text{AB}}{3\,.\,\rho}$, whence $\dfrac{s-\epsilon x}{\log\dfrac{5\,.\,\text{AB}}{3\,.\,\rho}} = 2C\epsilon$, and $2C = 3\cdot132$.

Second set of Resistance experiments, April 1875.

Case 1. Both poles on the edge of the circular disk.

$r = 13\cdot43$ centims., $\rho = \cdot163$ centim., $x = \cdot471$.

Number of experiments.	$2a$.	s.	ϵ.	$R = \dfrac{s}{\epsilon} - x.$	$\dfrac{R}{\log\dfrac{2a}{\rho}}.$
8	26·764	13·03	·9705	12·95	5·85
6	26·773	13·23	·9700	13·07	5·90
				Mean ... 5·87	

$$L = 4\log\frac{2a}{\rho}; \qquad \frac{R}{\log\dfrac{2a}{\rho}} = 4C; \quad 2C = 2\cdot935.$$

Case 2. One pole at the centre, the other on the edge (as at a, fig. 4).

$r = 13\cdot43$ centims., $\rho = \cdot163$ centim., $x = \cdot425$.

Number of experiments.	$AB = r$.	s.	ϵ.	$R = \dfrac{s}{\epsilon} - x.$	$\dfrac{R}{\log\dfrac{r}{\rho}}.$
3	13·43	9·37	1·035	8·63	4·504

$$L = 3\log\frac{r}{\rho}; \qquad \frac{R}{\log\dfrac{r}{\rho}} = 3C; \quad 2C = 3\cdot003.$$

Case 3. Both poles inside the disk at distances CA, CB respectively from its centre.

$r = 13\cdot43$ centims., $\rho = \cdot163$ centim., $x = \cdot38$.

AB.	CA.	CB.	s.	ϵ.	$R = \dfrac{s}{\epsilon} - x.$	$\dfrac{R}{L}.$
13·85	5·55	9·1	7·97	1·0595	7·14	1·63
20·00	10·9	9·1	8·94	1·0395	8·22	1·55
12·20	10·9	4·45	7·64	1·0625	6·81	1·58
7·25	4·5	3·62	6·45	1·0845	5·57	1·63
7·00	7·2	9·0	6·50	1·0850	5·62	1·61
9·00	0·0	9·0	7·17	1·0735	6·30	1·68
12·08	7·7	4·45	7·59	1·0650	6·75	1·64
15·12	7·7	7·7	8·04	1·0615	7·20	1·59
					Mean ...1·615	

The flow-lines for this case are indicated in Part. I. (Philosophical Magazine, S. 4. vol. xlix.) Plate X. fig. 9, where the circle with centre C corresponds with the edge of the disk.   In order to get the theoretical resistance for this arrangement, additional poles, of the same signs as A and B respectively, must be assumed at the points A′ and B′ ; so that, if the conducting sheet were unlimited, the circle with centre C would coincide with a line of flow : that is to say, A′ and B′ must be on the prolongations of the radii through A and B respectively, and at the following distances from the centre, $CA'=\dfrac{r^2}{CA}$, $CB'=\dfrac{r^2}{CB}$ (see Part I. §§ 36 and 38).   Taking A and B as the middle points of the circles of contact made by the two electrodes, we have (*loc. cit.*)

$$L= \log\frac{(AB)^2.\,AB'.\,A'B}{\rho^2.\,AA'.\,BB'}\,; \quad \frac{R}{L}=C\,; \quad 2C=3\cdot23.$$

The values of AB′ and A′B in the above expression were found by laying down on paper, to any convenient scale, the actual positions of the poles from the data AB, CA, CB, CA′, CB′, and then measuring the distances AB′ and A′B.   This is a quicker process than the corresponding calculation.

Case 4. One electrode at the centre, the other *touching*   Fig. 4. the edge (as at *b*, fig. 4).

$r=13\cdot43$ centims., $\rho=\cdot163$ centim., $x=\cdot38$.

Number of experiments.	$\begin{array}{c}AB\\=r-\rho.\end{array}$	s.	ϵ.	$R=\dfrac{s}{\epsilon}-x.$	$\dfrac{R}{\log\dfrac{r-\rho}{\sqrt[3]{3}\,.\,\rho}}.$
7	13·27	9·13	1·038	8·41	4·80

The poles being assumed to be the centres of the circles of contact made by the electrodes,

L becomes $3\log\dfrac{r-\rho}{\sqrt{3}\,.\,\rho}$; $\dfrac{R}{\log\dfrac{r-\rho}{\sqrt[3]{3}\,.\,\rho}}=3C$; $2C=3\cdot20.$

Case 5. Both electrodes *touching* the edge.

$r = 13\cdot43$ centims., $\rho = \cdot163$ centim., $x = \cdot38$.

Number of experiments.	AB.	s.	ϵ.	$R = \dfrac{s}{\epsilon} - x$.	$\dfrac{R}{\log\dfrac{AB}{\sqrt{3}\,.\,\rho}}$.
3	24·9	12·42	·983	12·25	6·30
1	16·8	11·58	·996	11·25	6·34
2	8·88	9·985	1·0065	9·54	6·36
2	10·68	10·59	1·016	10·043	6·36
2	22·0	12·34	·988	12·11	6·40
5	26·56	13·35	·969	13·40	6·79
				Mean...	6·49

In this case, A and B being again taken as the middle points of the circular electrodes,

L becomes $4 \log \dfrac{AB}{\sqrt{3}\,.\,\rho}$; $\quad \dfrac{R}{\log \dfrac{AB}{\sqrt{3}\,.\,\rho}} = 4C$; $\quad 2C = 3\cdot245$.

53. *Summary of mean values of* $2C = \dfrac{\log_e 10}{\pi \kappa \delta}$.

Date	1874.		1875.		Mean.
	No. of experiments.	2C.	No. of experiments.	2C.	
Case 1. Both poles on the edge	101	2·943	14	2·935	2·94
Case 2. One in centre and one on the edge	15	3·02	3	3·00	3·01
Case 3. Both inside the disk	9	3·13	10	3·23	3·18
Case 4. One in centre and one touching the edge	......	......	7	3·20	3·20
Case 5. Both touching the edge	......	......	15	3·245	3·245

It will be seen by this Table that the values of C deduced from the several sets of experiments are not quite constant. In explanation of the discrepancies, it is to be observed that the equipotential lines determined by the contact of the electrodes with the tinfoil did not agree quite strictly with equipotential lines due to the arrangement of poles assumed in calculating the values of L, except in the experiments (distinguished as Case 1)

where both the electrodes were on the edge of the disk. The differences between the results of these experiments and those with other arrangements of the electrodes are in the direction which would be caused by the actual departure from the theoretical conditions.

The discrepancy is greatest in Case 5, where both the electrodes *touch* the edge of the disk. In this case the additional poles A′ and B′, required in an imaginary unlimited continuation of the conducting sheet in order to make the circumference of the disk a line of flow, would be points outside the circumference, but very near to it and to A and B respectively. If, for an instant, we regard the electrodes as mere points whose distance from the circumference of the disk is very small compared with its radius or with their distance from each other, the equipotential lines for a short distance from A and B will be very nearly indeed a system of lemniscates (see Part I. § 36, at the beginning)—the node of the self-cutting curve, or lemniscate proper, being sensibly on the circumference. The circle of contact formed by each electrode may be regarded as approximately representing one branch of such a lemniscate, if, instead of taking its centre for the actual pole (A or B), we take a point nearer the centre of the disk by $(\sqrt{2}-1)\rho$, where ρ is the radius of the electrode. A recalculation of the experiments of Case 5 in this way reduces the mean value of 2C from 3·245 to 3·11; and a similar correction applied to Case 4 gives 2C = 3·10 instead of 3·20. Thus corrected, the results of these experiments agree fairly with those of Case 1.

54. Taking the mean value of 2C obtained with electrodes on the edge of the disk and multiplying it by $\dfrac{\pi}{\log_e 10}$, we get

$$4 \cdot 011,$$

which ought to be the resistance of a square centimetre of the tinfoil used. For comparison with the value so found, the same quantity was determined by measuring the resistance of an oblong strip of tinfoil, the electrodes being flat pieces of brass, firmly clamped down on the ends of the strip, and making contact with its entire width. Five measurements, made with different lengths of a strip 3 centims. broad, gave as the resistance of a square centimetre

$$4 \cdot 032,$$

the unit being, as before (see § 52), 0·00155 ohm; and five measurements, with a strip 1·5 centim. broad, gave

$$4 \cdot 029.$$

The mean of these determinations is

<center>4·031,</center>

which agrees closely with the value calculated from the experiments with the disks.

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55. It seems proper to point out in conclusion, that Professor J. Clerk Maxwell indicated so long ago as 1856 (Brit.-Assoc. Report, 1856, Trans. of Sect. p. 12 ; see also, for fuller developments, Camb. Phil. Trans. vol. x. p. 27) the fundamental idea of the method of treating questions relating to the flow of electricity in conductors of more than one dimension which was used in the first Part of this paper,—and also to mention that problems presented by finite plane conductors, in cases which become unmanageable by the simple geometrical processes employed by us (as, for instance, the case of rectangular conducting sheets), have been solved by Jochmann (Schlömilch's *Zeitschr. f. Mathemat.* 1865, p. 48) by a method founded on the superposition of the effects of separate poles, and by Heine (Crelle's *Journal,* 1874, vol. lxxix. p. 1) by an application of the geometrical principle of point-point correspondence (*Abbildung*) to Kirchhoff's results.

LVII. *Further Remarks on the " Crucial-Test " Argument. By* James Croll, *of H. M. Geological Survey*[*].

AS there is a slight discrepancy between Muncke's Table of the expansion of sea-water and that of Professor Hubbard, especially in reference to high temperatures, I have been induced to compare the results obtained by the use of the latter Table with that by the former ; and I find that they are almost identical. Both give 2 feet 6 inches as the amount by which the expansion of column B (lat. 23° N.) exceeds that of the equatorial column. Muncke's Table, as we have already seen, gives 3 feet 6 inches as the amount by which column A (lat. 38° N.) exceeds that of the equatorial, while according to Professor Hubbard's Table it is 3 feet 4 inches, being a difference of only 2 inches.

Dr. Carpenter objects[†] to my result on the ground that I have omitted the consideration of the inferior salinity of the equatorial column. Had I taken this into account, he thinks I should have found that it makes a difference in the opposite direction of about 1 foot in 1026, which would more than neutralize

the whole $3\frac{1}{2}$ feet of slope derived from temperature. I do not know upon what grounds he believes that the difference in salinity is so great between the equatorial and Atlantic columns. Certainly the researches of the 'Challenger' expedition do not warrant any such conclusion. It is true that there is an excess of salinity in the surface-water of the North Atlantic; but it does not extend to any great depth. This superior salinity of the warm upper stratum of the North Atlantic, it may be observed, is an additional evidence that the water is of Gulf-stream origin.

Through the kindness of the Hydrographer of the Admiralty I have been favoured with all the observations made in the 'Challenger' of the specific gravities of the Atlantic at intermediate depths, between surface and bottom. From these observations it will be seen that there is scarcely any sensible difference between the mean specific gravity of the equatorial and the two Atlantic columns.

The following Table shows the mean specific gravities of the three columns.

Atlantic.			Equatorial.		
Depth, in fathoms.	I. Lat. 38° 3′ N. Long. 39° 19′ W.	II. Lat. 20° 58′ N. Long. 22° 57′ W.	Depth, in fathoms.	III. Lat. 1° 22′ N. Long. 26° 36′ W.	IV. Lat 3° 8′ N. Long. 14° 49′ W.
	Specific gravity at 60°.	Specific gravity at 60°.		Specific gravity at 60°.	Specific gravity at 60°.
Surface.	1·02684	1·02685	Surface.	1·02616	1·02591
100	....	1·02732	50	1·02630	1·02658
150	1·02677	1·02658	90	1·02627	
250	1·02641	1·02642	100	....	1·02643
400	....	1·02609	200	1·02607	1·02620
500	1·02608	1·02600	300	1·02618	1·02610
1500	1·02607	1·02620	400	....	1·02629
			1500	1·02618	1·02613
Mean ...	·1·026211 = mean of column A.	1·02623 = mean of column B.	Mean ... Mean of two columns	1·026181 1·026202 =	1·026223 { mean of equatorial column C.

The mean specific gravity of the equatorial column, as proved by the two soundings III. and IV. in the Table, is 1·026202: and 1·026211 of sounding I. of the Table may be regarded as the mean specific gravity of the North-Atlantic column A; for the observations were made at a place on the same latitude and only about two degrees to the east of that column. Consequently

the specific gravity of column A exceeds that of the equatorial by only ·000009, a quantity which does not amount to 1 inch in 1500 fathoms! Sounding No. II. of the Table, made at a place a few degrees to the east of column B of the section, gives 1·02623, which may be regarded as the mean specific gravity of this column, and the more so as another sounding made in this region gives identically the same mean value. The difference between the equatorial column and the Atlantic column B in lat. 23° N. therefore amounts to only ·000028, or 3 inches in 1500 fathoms. It must of course be observed that, as the specific gravities in the Table are not taken at equal intervals, the mean of the figures does not represent the mean specific gravity of a column. The number of fathoms represented by each separate value must be taken into account in determining the mean value of the column. My former result, therefore, is not materially affected, even after I have thus taken into account difference of salinity, and computed the amount of expansion according to Professor Hubbard's Table. The surface of the Atlantic in lat. 38°, to be in static equilibrium, must be 3 feet 3 inches above that of the equator, and, in lat. 23°, 2 feet 3 inches above it.

It is perfectly true that, according to the gravitation theory, the ocean is never in a state of static equilibrium; but it must be observed that, as the surface-flow according to this theory is from the equator polewards, it is the equatorial column that is kept constantly below the level necessary to static equilibrium; hence were I to make allowance for want of static equilibrium I should make the slope greater than 3 feet 3 inches. Dr. Carpenter's objection that the force of my argument rests on the assumption that the sea is in static equilibrium, is based on a misapprehension of the problem; for in reality, by not making allowance for want of equilibrium, I give his theory an advantage which it does not deserve. Were the surface-flow from the North Atlantic to the equator, there would then be some grounds for his objection; for by leaving out of account the want of equilibrium I should be making the slope greater than it should be.

I submit that the foregoing is a satisfactory proof that the North Atlantic must be above the level of the equator; and if so, it shows the mechanical impossibility of a surface-flow polewards in that ocean resulting from gravitation.

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE ROTATORY POLARIZATION OF QUARTZ.
BY J.-L. SORET AND ED. SARAZIN.

THE angle which a plate of quartz, cut perpendicular to the axis. rotates the plane of polarization of light was determined by M. Broch[*], and more recently by M. Stefan[†], for rays of various refrangibilities, between the limits of the solar lines B and H. We have thought it would be interesting to extend those determinations to the ultra-violet rays by means of the spectroscope with fluorescent ocular formerly described by one of us[‡] We have also measured the rotation in the least-refrangible part of the spectrum, for the wave-lengths of the lines *a* and A, which, to our knowledge, had not previously been examined in this respect.

We have adopted the method of Fizeau and Foucault, which had been also employed by Broch and Stefan. The solar light, reflected horizontally by a metallic mirror (glass silvered), was concentrated by a convergent lens of 72 millims. aperture and about 1·15 mètre focal distance. A little before reaching the focus of the lens the pencil passed through a large Nicol prism, afterwards a quartz plate cut perpendicularly to the axis, then a second Nicol fixed upon a graduated circle, finally entering a spectroscope, the slit of which was placed at the focus of the lens.

The spectrum observed under these conditions is crossed, as is known, by black bands, the closeness of which increases in precisely the same ratio with the thickness of the quartz plate. By turning the analyzing Nicol, one of these bands can be brought into coincidence with such or such a line of the solar spectrum ; and from the angle through which it was necessary to turn it the rotation of the light of corresponding refrangibility can be deduced. In the ultraviolet part of the spectrum, observed with the fluorescent ocular, the black bands manifest themselves in the same manner ; and here we have been able to extend the determinations as far as the line N, which can be distinguished with sufficient distinctness by employing apparatus of optical glass and a prism of white flint glass. For the observation of the lines *a* and A it suffices to place before the slit of the spectroscope a plate of blue cobalt glass, which permits the extreme red to pass, absorbing the adjacent more refrangible radiations. These two lines are then readily distinguished, and, at least for *a*, the measurements can be made with as much precision as for the rest of the spectrum.

The results which we have obtained with a plate of left-handed quartz of 30·085 millims. thickness are recorded in the following Table :—

* Dove's *Repertorium*, vol. vii. p. 115 ; *Annales de Chimie et de Physique*, sér. 3, vol. xxxiv. p. 119.

† *Sitzungsberichte der Wiener Akad.* vol. l. p. 88.

‡ *Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* vol. xlix. p. 338, 1874 ; *Journal de Physique,* vol. iii. p. 253.

Lines of spectrum.	λ.	Observed angles of rotation.			Total number of observations.	Mean.	Calculated.	Difference.
A	760·0	12°·62	12°·68	......	20	12°·68	12°·78	+0·10
a	718·5	12·76	14·35	14·33	16	14·33	14·33	0·00
B	686·7	15·69	15·82	......	12	15·75	15·75	0·00
C	656·2	......	17·35	......	6	17·35	17·35	0·00
D	588·9	21·82	21·78	......	10	21·79	21·74	−0·05
E	526·9	27·68	27·57	......	10	27·61	27·55	−0·06
F	486·07	32·98 32·86	....	32·76 32·82	18	32·85	32·78	−0·07
G	430·72	42·67	......	42·59	12	42·63	42·69	+0·06
h	410·1	47·52	47·52	......	14	47·52	47·47	−0·05
H₁	396·8	51·21 51·12	51·23 51·16 51·33 51·10	51·37 51·37	46	51·22	51·22	0·00
L	381·9	......	56·05 55·71	......	12	55·88	55·80	−0·08
M	372·7	......	......	59·03	8	59·03	58·99	−0·04
3rd strong line of the group M	372·0	....	59·18 59·20	59·31	21	59·24	59·24	−0·00
N	358·5	64·76	64·73	64·28	28	64·41	64·44	+0·03

The third, fourth, and fifth columns give, in degrees and decimal fractions of a degree, the values of the angle of rotation as obtained in the three different series of observations, referred to a plate of quartz 1 millim. thick. The second series of experiments was made with two prisms to the spectroscope from A to E, with only one for the more refrangible lines; series I. and III. were made with but one prism. The numbers given are most frequently the mean of six observations, but sometimes of four, eight, twelve, or sixteen. The values given in the eighth column were calculated by Boltzmann's formula, as we shall subsequently see.

The accordance of the different series varies a little, according to the lines, which are not all of equally easy adjustment. For the line H₁, for example, there is some divergence, because the nearness of the line H₂ somewhat impedes the observation; and it is the same with the numerous lines of the group L. The lines M are very distinct; but the feeble luminous intensity of N considerably lessens the precision.

The results recorded in the seventh column approach very nearly those obtained by MM. Broch and Stefan, viz.

	P.	C.	D.	E.	F.	G.	H.
Broch	15°·30	17°·24	21°·67	27°·46	32°·50	42°·20	
Stefan	15·55	17·22	21·67	27·46	32·69	42·37	50°·98

Nevertheless our numbers a little exceed those of M. Stefan. These slight divergences may, we think, be attributed to two causes :—

(1) Our experiments must have been made at a notably higher temperature ; for we worked on some of the very hot days of this summer (outside temperature 20° to 25° C.), and, besides, the sun's rays concentrated by a lens upon the plate of quartz necessarily raised its temperature. Now it has been shown by Von Lang (*Sitzungsberichte der Wiener Akademie*, vol. lxxi. April 1875) that the rotatory power of quartz slightly increases with its temperature ; and, to express the increment, he gives the formula

$$\phi_t = \phi_0(1 + \cdot 0000149 t),$$

where ϕ_0 and ϕ_t are the angles of rotation at zero and at $t°$. By admitting a difference of 20° between the temperature of M. Stefan's experiments and that of ours the difference between the results would be reduced about 3 thousandths.

(2) The plate of quartz we use shows a slight imperfection in its cutting, with respect to parallelism of the two faces and the direction of the axis ; and the rays did not pass through it quite normally : they were rendered convergent by a lens of long focus. This little cause of error also tends to augment the numbers obtained.

For the expression of the angle of rotation ϕ as a function of the wave-length λ, the following formula has been proposed—

$$\phi = -A + \frac{B}{\lambda^2},$$

in which A and B are two constants. This formula, which accords in a manner but little short of satisfactory with the observations made between B and H, becomes inaccurate between more extended limits. When the values of A and B are calculated from those of ϕ for lines a and M, it is found that, for rays of intermediate refrangibility, the values given by the formula are constantly higher than the numbers observed ; the divergence exceeds 1 degree for the line G. On the contrary, for the lines A and N the calculated values are lower than those resulting from observation.

Starting from the idea that the rotation should be zero for an infinite wave-length, Boltzmann has proposed the formula

$$\phi = \frac{B}{\lambda^2} + \frac{C}{\lambda^4} + \frac{D}{\lambda^6} + \cdots$$

He has shown that, reduced to its first two terms, it conducts to values well accordant with the observations of M. Stefan between B and H. We have found that it likewise agrees very satisfactorily with our results. Calculating B and C from the numbers we have obtained for the lines a and M, we get

$$\phi = \frac{7 \cdot 10533}{10^6 \lambda^2} + \frac{0 \cdot 151227}{10^{12} \lambda^4},$$

from which are deduced the numbers of the eighth column of our Table.

By applying a few modifications to our apparatus we expect to secure greater precision, and to extend our measurements beyond the line N.—*Comptes Rendus de l'Académie des Sciences*, vol. lxxxi. pp. 610–613.

ON THERMIC EQUILIBRIUM AND HEAT-CONDUCTION IN GASES; AND ON THE INTEGRATION OF PARTIAL DIFFERENTIAL EQUATIONS OF THE FIRST ORDER. BY PROF. LUDWIG BOLTZMANN.

At the sitting of the [Vienna] Academy of the 14th October, Prof. Boltzmann presented three memoirs. The first, " On the Thermic Equilibrium of Gases when acted on by external Forces," contains the demonstration that, even in the case of the operation of external forces, a function E exists of which the value cannot increase in consequence of the molecular motion, and consequently must have a constant minimum value ; indeed this function is perfectly identical with that which possesses this property when no external forces are present ; only the subsidiary conditions are different. In order to furnish the demonstration, in the first place the general partial differential equation for the change of state in consequence of the molecular motion is developed ; it contains some terms which also occur when no external forces act, and others which result from the presence of the external forces. It is then proved that the latter terms supply a vanishing quantity to the expression obtained for the differential quotient of the function E according to the time, whence it follows that the laws of the variation of this function with the time undergo no change by the operation of the external forces. But the subsidiary conditions are different, inasmuch as the equation of the *vis viva* must now contain also the ergal of the external forces. By means of this proposition the distribution of state which enters in the case of thermic equilibrium can with facility be determined. In the second memoir, " Remarks on the Conduction of Heat by Gases," some theoretic considerations are attached to the experimental investigations of Stefan, Kundt, Warburg, and Winkelmann. Stefan already remarked that these observations seemed to indicate that the intramolecular motion takes part in a slight degree, as Maxwell assumed, in the heat-conduction. This is here further elucidated ; and it is shown that perfect accordance with experiment is attained by assuming that the contribution furnished by the intramolecular motion to the heat-conduction amounts to only $\frac{3}{13}$ of what it would be according to Maxwell's hypothesis. The author remarks that this is much less than what the intramolecular motion in consequence of the mere diffusion of the molecules would contribute to the heat-diffusion if the proportion of the *vis viva* of the progressive and intramolecular motions were in each layer the same as in a uniformly heated gas of the same temperature. The observations hitherto made appear, therefore, to favour the opinion that such is not the case. It would hence follow that the constancy of heat-conduction is not entirely independent of the thickness of the conducting layer. In conclu-

sion, very general differential equations for the variation of the *vis viva* of the progressive and intramolecular motions are advanced.

The third memoir, "Integration of Partial Differential Equations of the First Order," contains a new proof of Jacobi's method of integration of that kind of partial differential equations. As is well known, these differential equations are integrated by first integrating a more general system of simultaneous partial differential equations, and then determining the arbitrary functions so as to satisfy also the differential equations originally given. In order completely to determine the unknowns, more limiting conditions must be added to the former more general system than to the one originally given. In the memoir the demonstration is furnished that, by means of Jacobi's integration-method, the limiting conditions attached to the more general system coincide with one required by the given differential equation, whence it at once follows that Jacobi's solution coincides with a solution of the given differential equation.—*Sitzungbericht der k. Akademie der Wissenschaften in Wien, math.-naturw. Classe,* 1875, No, 20.

ON SOUNDING FLAMES.

M. C. Decharme finds that persistent and varied sounds may be obtained by allowing a current of air to impinge on a jet of common illuminating-gas issuing from a tube 3 to 5 millims. in diameter, and forming a flame 30 to 50 centims. high. The air is conveyed in a similar tube; and the character of the sound will vary according to the part of the flame struck, the pressure of the air, and the ratio of the diameter of the tubes.

When the jet of air, striking the flame near the top, gradually descends to within about a decimetre of the orifice, we see the column of flame first divide, lower itself, then twist under the jet, envelope it and let it pass, surrounding it with a blue flickering flame; a continuous tearing sound is then heard from this luminous veil. When the jet reaches within 2 or 3 centims. of the orifice of the gas (the air-tube being held horizontally and being directed toward the flame), a loud hissing is produced. Finally, when the tubes touch, the hissing becomes harsher, or, if the pressure is small, becomes a very clear and agreeable musical sound.

The experiment also succeeds well with a Bunsen burner after closing the air-holes. It is needless to add that no sound is produced with the simple gas-jet if not lighted. By varying the conditions, as the nature and pressure of the gas and air, the position, diameter, form, and nature of the tubes, various modifications of the sounds, and of the shape and colour of the flame, are produced. The rapid motions are readily analyzed by a revolving mirror. It would appear that the explanation of these facts depends not merely on the mechanical, but in a great measure on the chemical action of the air.—*Comptes Rendus,* vol. lxxx. p. 1602; Silliman's *American Journal,* November 1875.

LIX. *On the Polarization of the Light of the Sky.*
By R. H. M. Bosanquet, *Fellow of St. John's College, Oxford**.*

THE subject of the polarization of the light of the sky has
been regarded from two principal points of view—namely,
as matter of observation, and as the subject of a theory based on
Tyndall's experiments with attenuated vapours and smoke. The
comparison of the observations of the sky with the experiments
has not yet been carried out to any purpose, so far as I am aware.
It will be the aim of the present communication to discuss the
results of observation of the sky in the light of certain elemen-
tary indications afforded by the experimental treatment. A
few words will be said on the general theory. The subject of
the construction of polarimeters will also be noticed, and some
remarks made on the principles on which the interpretation of
their indications may be made to depend.

By far the most complete treatment of the matter by obser-
vation of the sky is that of Brewster. His map of the lines of
equal polarization was published in Keith Johnston's 'Physical
Atlas,' accompanied by a short explanation. It has never, so
far as I know, been unfavourably criticised, and is the authority
to which probably any one interested in the subject would look
in the first instance. On inspecting this map it appeared to me
so strange, that I thought it desirable to investigate the matter
more closely; and I have now to present the considerations
which lead me to the conclusion that it must be almost entirely
rejected, not only as an exposition of the actual state of things,
but even as a representation of the results of Brewster's own
observations.

Brewster's observations were subsequently published in the
'Transactions of the Royal Society of Edinburgh,' 1864, vol. xxiii.
p. 213, and in the Philosophical Magazine for August 1865, p. 118.

They appear to have been taken straight from the note-book, if we may judge from several uncorrected mistakes; this, of course, materially enhances their value as observations. I have no doubt that they constitute by far the largest and most reliable body of evidence accessible on the most interesting parts of the subject; also it is clear that they actually were the evidence on which his map and discussion were founded.

The considerable work of Rubenson (*Mémoire sur la Polarisation de la lumière atmosphérique*, Upsal, 1864) is devoted almost entirely to the variations of the maximum intensity of the polarization of the sky, the time of day and year being connected with it by empirical formulæ. Although his instrument was almost of an astronomical character as regards the attempt at accuracy, and his series of observations are very extensive, these are not for the most part applicable to the determination of the points to which I have so far principally attended; in a more advanced stage of the subject his work may probably be of value.

The summary of general results, as stated by Brewster, refers only to the plane through sun, zenith, and observer; and it is clear from the observations that points in this circle, and the point in the horizon at right angles to it, were alone generally observed. The normal arrangement of the polarization is taken to be when the sun is in or near the horizon (generally setting).

Polarization is said to be positive when light is polarized in a plane through sun, point, and observer; negative when polarized at right angles to that plane.

Thus, in Tyndall's experiments, the polarization of the first stage, where the blue light first appears, and the polarization is parallel to the beam, is positive; in the subsequent stage the polarization of rays forming an acute angle with the beam is negative.

Now Brewster and all the observers are agreed in this, that the polarization of the region in and about a great circle at right angles to the sun is always positive, and that the position of points of maximum positive polarization is always at right angles to the sun.

They are also agreed that there are in the plane through sun, zenith, and observer at least three neutral points:—the point of Arago, between the zenith and antisolar point; the point of Babinet, between the zenith and the sun; and a point called by Brewster's name, below the sun.

Also that between Arago's point and the antisolar point the polarization is generally negative; that between Babinet's point and the sun the polarization is negative; and between the sun and Brewster's point the polarization is negative; though the observation of the negative polarization in the last case, and

consequently of Brewster's point, appears to be of extreme difficulty.

Brewster himself has occasionally seen a fourth neutral point in the neighbourhood of the antisolar point; and there exist isolated observations of other points. But for the present purpose it will be sufficient to consider the three above named, as they stand on quite a different ground as to evidence from the others, and they are those on which Brewster founds his discussion.

Brewster assumes for his normal positions:—sun in horizon; altitude of Arago (above horizon opposite sun) = altitude of Babinet (above sun) = depression of Brewster (below sun) = 18° 30'. Of course Brewster's point would not really be visible under these circumstances; but it is assumed that that would be its position if it were visible, for the sake of obtaining a normal configuration. I may here say that I have not been able to find out how Brewster determined this value 18° 30'. I attribute a certain weight to it, and to the assumption that the points are symmetrically situated (under normal circumstances of course), as representing Brewster's opinion on points of fact; but I cannot get the number, or the symmetry, out of the observations. My impression is that he assumed the symmetry, which no doubt exists, at all events as a rough approximation, and then took some sort of mean of all the observations. For the present I retain Brewster's numbers, as they afford a convenient presentation of the principal point at issue, and are, in all probability, subject to the assumptions made, not very wide of the truth.

Measure of polarization.—Brewster employs a peculiar measure of the polarization of partially polarized light, founded on a hypothesis which, I suppose, few now remember*. Referring to Phil. Trans. vol. cxx. p. 76, we see that Brewster's ϕ is what I shall call "the polarimeter angle."

We may remark that if a ray of polarized light is decomposed by a doubly refracting quarter-wave plate cut parallel to the axis, into elliptically polarized light, then, if ϕ be the angle between the principal section of the plate and the plane of polarization, the intensities of the components of the principal vibrations, according to Malus's law, will be $A^2 \sin \phi^2$ and $A^2 \cos^2 \phi$; and if we put

$$\text{Polarization} = \frac{\text{Difference of principal intensities}}{\text{Sum of principal intensities}},$$

we have

$$\text{Polarization} = \frac{\cos^2\phi - \sin^2\phi}{\cos^2\phi + \sin^2\phi} = \cos 2\phi.$$

* Phil. Trans. 1830, pp. 69, 133.

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If the light thus polarized, with known values of ϕ, be examined with a glass-bundle polarimeter, it is possible to express experimentally the inclinations of the glass bundle in terms of ϕ, where $\cos 2\phi$ is the numerical measure of the polarization. This is Rubenson's method. The change of sign corresponds conveniently with the expression " positive " or " negative " polarization.

Now Brewster's measure of polarization is R, defined by the relation $R = \phi - 45°$ (Ed. Trans. xxiii. 233, Phil. Trans. 1830, 136). Hence the polarization indicated by R is $\cos 2(45° + R)$, or, disregarding sign, $\sin 2R$; so that $R = 45°$ represents complete polarization, $R = 0$ common light. If the polarization were negative, it would be represented by negative values of R.

Brewster's normal value of the maximum zenith polarization is about 30°; and he notices that the horizontal polarization at right angles to the sun (minimum maximorum) is somewhat less. I have not been able to find out how he got from the observations the constants he employs in either case; but they represent the observations very fairly. We are now able to enter on the method employed by Brewster for the construction of his maps. It is explained, both in the text in Keith Johnston and in the Philosophical Magazine, 1847, vol. xxxi. p. 451, where a slightly different version is given; so there is no room for doubt that the procedure about to be described was that actually employed.

First was considered the section of the sky in the plane of zenith, sun, observer, the normal positions being as given above, and the maximum polarization being about 30°. This distribution was represented by the expression $33\frac{1}{2}°$ sin D sin D$'$, where D, D$'$ were the angular distances from the two neutral points. In the zenith (where $D = D' = \dfrac{\pi}{2} - 18° 30'$) this gives $30\frac{1}{8}°$ nearly. The law of variation was not in any way determined; but the expression gives the required value in the zenith, and vanishes at the neutral points.

Having regard to sign, this expression represents the distribution of polarization in the plane of sun, zenith, and observer sufficiently well, except that we shall see reason to believe that the polarization must vanish at the sun and antisolar point. But it is necessary to make the convention that D, D$'$ shall be taken negative when reckoned in the direction opposite to the zenith. The following figure shows more clearly what is meant; the dotted lines represent negative, the continuous line represents positive polarization.

Fig. 1.

Zenith.

Arago- - - - - - - - - -Babinet.

Sun

Brewster.
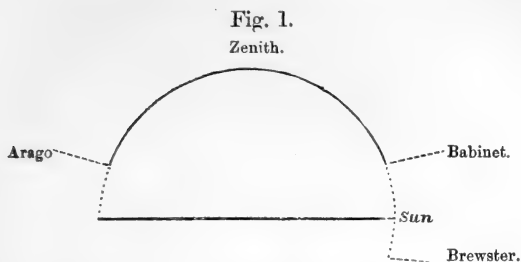
But the application of the formula to points out of the plane of sun, zenith, and observer involves a pure assumption, viz. that the neutral points are centres, or their radii vectores axes of the polarization, so that D, D′ may be applicable to distances from the neutral points measured in any direction. For this assumption there is no foundation whatever; and it leads to the entire disregard of the convention of sign above alluded to, and to the representation of the polarization in the negative region as similar in character, and, indeed, as continuous with, that in the positive region. The adoption of this assumption by Brewster led to a distribution of the polarization entirely inconsistent both with his own observations and conclusions, and with those of all other observers, as well as absurd in itself. In fact this is probably the only case where geometers have for a considerable time been content to acquiesce in the statement that the boundary between two regions on the surface of a sphere is a point, or, as Rubenson puts it, " Ces deux espaces du ciel, caractérisés par leur polarisation contraire, sont séparés l'un de l'autre par un intervalle très restreint, dont la lumière ne montre pas la moindre trace de polarisation, et que l'on appelle *point neutre.*" (P. 3.) I have to thank Prof. H. J. S. Smith for stating this point with remarkable clearness at the meeting of the Ashmolean Society, at which this paper was read.

Brewster made this assumption—namely, that the above formula was applicable in whatever direction D and D′ were measured. Then in applying the case to the point in the horizon at right angles to the sun, of which he had observations, he found that the formula gave a polarization greater than that in the zenith, whereas his observations showed that it was less; in fact the formula was in no sense applicable to that point at all. But to correct the discrepancy he introduced a term, $-6° 34′$ (sin z sin A); this reduced the value for the horizon maximum to $27°$ very nearly, which is within the limits of the observations, though still rather large, and did not affect the plane of sun, zenith, observer, for which A $=0$. (A $=$ azimuth, $z =$ zenith-distance.) It was from this formula (R $= 33\frac{1}{2}°$ sin D sin D′

Mr. R. H. M. Bosanquet *on the*

$-6° 34' \sin z \sin A$) that Brewster's maps were constructed; and the nature of the result will be best seen from the following sketch, which represents the general course of the lines of equal polarization on his two maps. The projection of I. is on the plane of the horizon, of II. on a vertical plane normal to the line to the sun. The projection is such that equidistant small circles parallel to the plane of projection are equidistant on the projection.

Sketch of Brewster's Maps of Lines of equal polarization.

Fig. I.

Arago'
Point.

Babinet's
Point.

Fig. II.

Babinet's
Point.

Sun

Brewster's
Point.
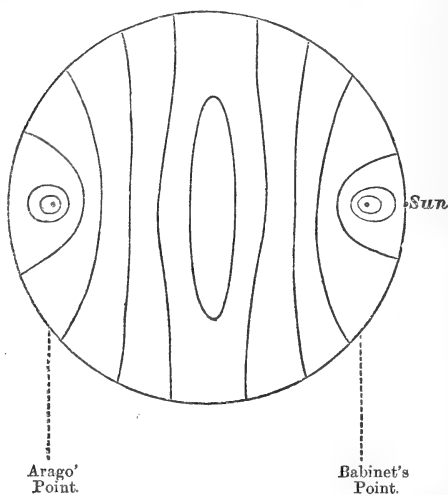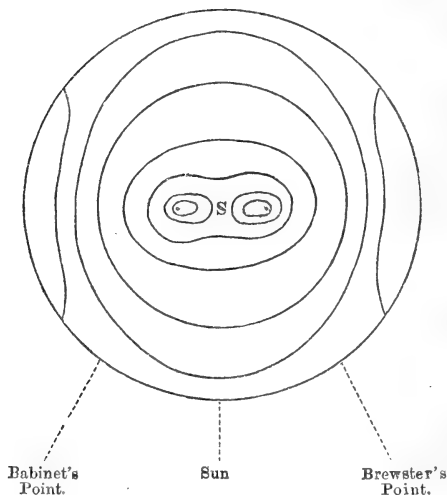
We see now that these maps disregard entirely the existence of a negative region. For instance, the first line, corresponding to polarization $R = 1°\cdot 6$, goes right round each point, through both positive and negative regions. Hence each of the neutral points has positive polarization both above and below it, which is contrary to all the observations.

Again, suppose a point taken anywhere in the positive region, and another in the negative. It is clear that, on any line joining these, the polarization must pass through a zero value on changing sign. Hence there must be a neutral line or region separating the positive and negative regions. The extreme difficulty of the observations may be the reason why this has not been established by observation. But it is clear that it must be so; and traces of the fact are found here and there in the observations. I will mention some cases.

Edinburgh Trans. xxiii. p. 214, June 10 :—" At $3^h 47^m$, when the sky was everywhere pure and free of clouds, the neutral line of the polarimeter was curved above the land horizon. At $6^h 32^m$ the curvature was greater above the sea horizon, the deviation increasing towards the horizon."

1842, Feb. 21, at $12^h 39^m$:—" Neutral line convex towards the sun in western horizon."

In the figure (p. 213) of the bands in the polarimeter, the appearance indicated is that of a horizontal neutral line.

Rubenson (p. 144) swept the horizon immediately after sunset; at a very small distance from the sun he recognized a well-defined neutral point. No azimuth is given; and this is the only observation he gives of the phenomenon.

Probably the difficulty of the observations may be the reason why no observer has searched methodically for neutral lines, but always for points; these may be merely the intersection of the neutral line with the circle in which the instrument sweeps. Rubenson was frequently unable to find any of the neutral points, and has no detailed observations of them. We also find that Babinet regarded the observation both of his own point and of Brewster's as of extreme difficulty.

Before proceeding to suggest a more consistent interpretation of the data, I will advert shortly to the principal points established in Tyndall's experiments, and endeavour to gain some general principles for our guidance.

Stage I. Vapour of minimum density. Blue colour. Polarization positive. Light emitted normally to the illuminating beam perfectly polarized.

Stage II. Increased density of vapour, and probably increased size of particles. Blue passes over into white.

(α) Light emitted normally is partially polarized positively

(in the plane of the beam); the portion polarized negatively (passing through a Nicol which stops the positively polarized light) affords the phenomenon called by Tyndall residual blue.

(β) If a point on the beam be observed from positions such that the light examined diverges at an acute angle with the incident beam, a position can be found in which the emitted light is neutral, or devoid of polarization.

(γ) If a point on the beam be observed from positions such that the light examined diverges at a more acute angle than in the last case, the light is polarized negatively, or at right angles to the beam.

In Stage II. there is a progressive change, which will be as follows, if we suppose the density, or, as we may say with great probability, the mean size, of the diffracting particles continually to increase.

(α) If the beam be regarded normally, the light, which throughout Stage I. was perfectly polarized, ceases to be so, the proportion of polarized light thus normally emitted diminishes, and eventually the light normally emitted is neutral; this I take to mark the conclusion of Stage II.

(β) The angle at which neutral light is emitted, which throughout Stage I. was evanescent, gradually increases, till at the conclusion of Stage II. it becomes a right angle, viz. when the light normally emitted is neutral.

(γ) The negative polarization of light emitted at angles less than the neutral angle, increases continually in intensity. Towards the close of Stage II. this negative polarization is stronger than the normal positive polarization. According to the experiments the direction of emission for maximum negative polarization seems always to have formed a very acute angle with the beam.

Stage III. A third stage appears to have been reached, in which the polarization was everywhere negative. Here,

(α) The polarization of light normally emitted, after vanishing at the conclusion of Stage II., reappears with changed sign; *i. e.* it is now negative.

(β) No neutral light is emitted in any direction.

(γ) The direction of emission of the light with maximum polarization forms a very acute angle with the beam.

As these deductions are stated in a different form from that employed by Tyndall, I must point out how they are derived from his facts.

As to Stage I. there is no difficulty. See 'Radiant Heat,' p. 353. (The sentence at the end of (I.) is rather a stumbling-block. "The direction of maximum polarization is at right angles to the beam." This means "The direction *of emission* for maximum polarization.")

The description of the change of the direction of emission for maximum polarization in (7) *l. c.* is as follows:—" As the actinic cloud grows coarser in texture, the direction of" (emission for) " maximum polarization changes from the normal, enclosing an angle more or less acute with the axis of the illuminating beam." As this stands it would convey the impression that this direction changed gradually from the normal. There are no detailed experiments with vapours on this point; and they would be very difficult to execute, on account of the influence of the oblique incidence on the glass tube on the polarization. But here step in the experiments on smoke, recorded p. 437 (Radiant Heat). No doubt there is an assumption in supposing that these experiments are comparable with those on vapours; all one can say is, that the smoke experiments appear to correspond with those on the vapour so far as direct observation of the latter is possible, and also generally in other respects. And, according to the smoke experiments, there is no doubt that what takes place is, not a gradual change from the normal position for maximum polarization, but a fading away of the positive polarization normally emitted, and an origin and increase of a negative polarization, the greatest value of which is in light emitted at a very acute angle to the beam. Tyndall generally estimates this angle of emission with the beam at $12°$. But from his arrangements it seems doubtful whether this is not simply the smallest angle that could be conveniently observed. This point requires more attention. The observations seem to indicate an increase of negative polarization close up to coincidence with the beam —a result which would appear impossible from the symmetry.

In (8) *l. c.* there is a slight difficulty. " In passing from section to section of the same cloud, the plane of polarization often undergoes a rotation of $90°$." It appears from the next experiment (air, benzol, and hydrochloric acid) that the action occasionally distributed itself unequally through the tube; and certain portions presented white clouds (*i. e.* an advanced stage of action), while others remained blue (*i. e.* in a comparatively early stage). The plane of polarization of the blue segments was parallel to the beam (Stage I.), while the plane of polarization of the white segments was at right angles to the beam (Stage III.). The causes of this distribution of the action, where advanced and retarded points lie side by side, do not concern us here.

The experiments on smoke at p. 436 (' Radiant Heat ') are for our present purpose of the greatest importance. In describing them I shall avoid the phrase " neutral point." It appears to involve *sub silentio* the assumption that the different points of the beam possess different properties. This does not appear to

be the case when the cloud is homogeneous. (P. 435.) Tyndall observed the track of a beam in air 18 feet long. The normal polarization was the same throughout the whole length of the beam.

What is to be considered is really the nature of the emission at any given angle with the axis of the beam. Instead, therefore, of regarding the eye as fixed and the point of the beam observed as variable, it is convenient to suppose a fixed point of observation in the beam, and to imagine the position of the eye varied, so as to observe rays making different angles with the axis of the beam. We thus obtain the distribution of polarized light as emitted from a fixed point of the beam, and instead of the neutral point of Tyndall we have a neutral angle.

I will take the description of three of Tyndall's experiments from p. 437 ('Radiant Heat'). The laboratory was filled with smoke of some kind, and the electric beam sent through the smoke. The neutral angle and position of maximum polarization were observed, and the smoke gradually allowed to disperse. The observation of the neutral angle was then repeated at intervals. Thus the earlier observations in each experiment correspond to the more advanced stage (coarser particles), and the termination of each experiment to the less advanced stage (finer particles). Stage I. does not seem to have been reached; and this might have been expected, as the finest particles of smoke are probably coarser than the attenuated vapours of the exhausted tubes with which the phenomena of Stage I. were observed. I must remark that in the account of these experiments the actual character of the polarization (positive or negative) is not stated. I have assigned this by the consideration that, in the most attenuated condition of the smoke, the polarization of light emitted normally is always positive.

	Exp. 1. Incense.	Exp. 2. Gunpowder.	Exp. 3. Resin.
Angle of emission for initial maximum polarization (negative).	12 or 13°	......	12°
Initial neutral angle	66	63	{ No neutral emission.
Successive values of the neutral angle as the smoke cleared away	63 54 49 43 33	50 47 42 38	90 Less than 90

Final direction of emission for maximum polarization (positive) normal to beam.

The third experiment shows the passage from Stage III. (in which the polarization is everywhere negative) into Stage II.

(where the positive and negative regions are separated at a certain angle).

It is to be regretted that no observations are given, by the accomplished observer in question, of light emitted from the beam at angles obtuse to the incident direction. It thus remains uncertain, so far as these observations go, whether the phenomena are continuous through the two right angles on one side of the beam, or symmetrical in the two quadrants. The only indication we can gather is, that the neutral emission seems to have disappeared altogether in stages beyond that in which the neutral angle is 90°.

It is of course conceivable that the neutral angle might have continued to increase beyond 90°. And if it is true that it does not, there is a strong presumption that symmetry in the two quadrants subsists at least up to the beginning of Stage III. The question is easily seen to be the same as whether, under normal circumstances, in the sky, the points of Arago, Babinet, and Brewster are equidistant from the antisolar point and the sun respectively. As I have said, it is clear from the observations that this is so to a rough approximation ; and only further observations, or especially further experiments, can decide in a more accurate manner.

We may embody the results of these experiments in the remark that there exists a relation between the polarization observed normally and the neutral angle, of the following nature:—

Normal polarization.	Neutral angle.	
1	0	Stage I.
diminishes	increases	} Stage II.
0	90°	

The first and last relations may be called the terminal conditions.

Before leaving Tyndall's experiments, we may notice the remarkable effects he alludes to on p. 438.

Observing the ring-system in common air (normally to beam), puffs of smoke or of steam are introduced : the brilliancy in both cases is greatly intensified ; but in the case of the steam the ring-system is reversed. The actual characters of polarization are not given ; but as the normal polarization in common air (dust) is positive, it is to be assumed that the smoke was also positive, and the steam negative. In a note a suggestion of Sir John Herschel's is mentioned as to the cause being a change from polarization by reflection to polarization by refraction. The earlier writers might have used this phrase simply to describe the phenomenon. I do not pretend to offer any criticism as to the merit of the suggestion ; but I must point out that the

effect is perfectly in accordance with all the other experiments; and the hypothesis which explains them, whatever it may be, will also explain this.

We have seen that, with three kinds of smoke, the variation of the stage of advancement of the action is of a remarkably limited character. With the incense and gunpowder the action was entirely within Stage II., and the greatest variation of the neutral angle was only $33°$ in the first experiment and $25°$ in the second. It was only with the fumes of resin, "rendered so dense as to be very irritating to the lungs," that Stage III. was entered upon, and this, according to the description, by only a little step. This appears to indicate a certain uniformity in the size of the particles which constitute the smoke employed; and, at all events, the inference may be safely drawn that smoke produced in any ordinary way is not likely to proceed so far as the end of Stage II. Now what happened was that the track of the beam in ordinary air was observed, not with a polarimeter, which would have given some sort of measure of the ratio of polarized to common light, but with a selenite and Nicol, a combination which gives no measure of the kind. For instance, supposing light to contain half polarized and half common light. Then, if the light be very faint, it will give with selenite and Nicol a faint ring-system; if the light be stronger, the ring-system will be stronger; in short, the indication depends as much on the strength of the light as on its percentage composition. The indication of the polarimeter, on the contrary, is, or ought to be, independent of the strength of the light. Suppose, then, that into the faint luminosity of the beam in air a puff of smoke is thrown in a state corresponding to the initial condition of experiments 1 or 2. The normal emission is greatly increased, for there is so much more matter to disperse the light; and the polarization is still positive, for the state of the smoke is still below Stage III. There is, then, so far no need for additional explanation; the increased light amply explains the increased brilliancy of the ring-system.

When vapour is employed, it needs little consideration to see that the particles may be of any size. In fact, in the experiments with tubes, it was the natural course for them to begin, in Stage I., much smaller than the smoke particles ever are, and, passing rapidly through Stage II., to indicate by the negative polarization emitted normally, as well as by their coarser physical structure, their rapid growth in size.

What seems to make it clear that the transparency is at all events not a necessary condition for Stage III. is, that it was attained with the resin smoke, the particles of which were no doubt opaque.

We will now return to the sky.   If any point be observed, we have only to imagine a series of illuminating beams drawn from the sun to the points of the atmospheric track along which the eye is directed, and we have the atmospheric phenomena referred, according to the analogy first indicated by Tyndall himself, to the same origin as the experiments.

The first remark that this leads to is, that as in the experiments the appearances are symmetrical all round the beam, so in the sky the appearances must be symmetrical about the line to the sun, allowing for the variation in condition between those beams that lie overhead and those that lie nearer the horizon. We are thus led to the assumption of the line to the sun as an axis instead of the two axes of Brewster drawn to the neutral points.

In the first instance we may regard as a small quantity the variation between the maximum polarization in zenith and horizon.   Brewster allows only $(30° - 27°) = 3°$ of his measure R for the normal difference.

Employing, then, Brewster's data, we may represent R to a first approximation thus :—

$$R = 33\tfrac{1}{2}° \sin (\theta - 18\tfrac{1}{2}°) \sin (\theta + 18\tfrac{1}{2}°),$$

where θ is the angular distance from the sun of the point observed.   This expression has the same value as Brewster's first term throughout the plane of sun, zenith, observer, but represents the rest of the sky as if traced by the revolution of that plane about the line to the sun.

In the light of Tyndall's experiments we now see that the diminution of maximum polarization from zenith to horizon may be regarded as due to a small advance in the stage or mean size of the diffracting particles.   It will be then best expressed by a variation of the maximum polarization due to the zenith-distance ; and the formula becomes

$$R = (33\tfrac{1}{2}° - 3\tfrac{1}{2}° \sin z) \sin (\theta - 18\tfrac{1}{2}°) \sin (\theta + 18\tfrac{1}{2}°).$$

The constant $(3\tfrac{1}{2}°)$ gives pretty closely the same horizontal value $(27°)$ as Brewster's formula.   The observations do not admit of any considerable accuracy in fractions of degrees.   The variation from zenith to horizon deduced from some of Rubenson's sets, and reduced to Brewster's measure, is about 8°; and this really accords better with some of Brewster's observations than the smaller value he employs.

But, further, it is clear from the symmetry that the polarization observed along the line to sun, excluding the effect of indirect light, must be *nil*; for there is no reason why it should be in one direction more than another.   We must therefore introduce a factor which will vanish along the solar line without

affecting the values in zenith and horizon at right angles to sun, or the evanescence at neutral points.   The formula now becomes

$$R = (33\tfrac{1}{2}° - 3\tfrac{1}{2}° \sin z) \sqrt[n]{\sin \theta} \, . \, \sin (\theta - 18\tfrac{1}{2}°) . \sin (\theta + 18\tfrac{1}{2}°),$$

which represents all Brewster's results, as well as the necessary conditions inferred from the consideration of the experiments.

n is here some large number which cannot be determined without more evidence.   The object is to get a factor which is very nearly unity except for evanescent values of θ.

For the sake of comparison, the following sketch (figs. III. & IV.) is drawn on the same arrangement as Brewster's maps, showing the general course which the lines of equal polarization must take under these circumstances.   Continuous lines represent the course of given values of positive polarization, dotted lines of negative.

The interval between the continuous and dotted lines represents the neutral region, in which the neutral points lie.

The projections of III. and IV. are the same as Brewster's I. and II. above given.

I have said that the symmetry requires the evanescence of the polarization at the sun and antisolar point; and I have represented this consideration in the above empirical formula by the factor $\sqrt[n]{\sin \theta}$, where n is a large indeterminate number.   I have also pointed out that Tyndall's experiments contain no evidence of this phenomenon.   There are, however, several observations of the sky which I cannot attribute to any thing else.   About the antisolar point, I do not consider that the observations are of any value, as, strictly speaking, the direct rays of the sun can never fully reach the antisolar point when it is observed; but a neutral region immediately surrounding the sun has been observed on several occasions.   Babinet observed it as a neutral point below the sun; Rubenson observed it above.   Babinet's account is very clear (*Comptes Rendus*, 1846, No. 2, p. 233).   Karsten, in *Fortschritte der Physik*, ii. (1846) p. 187, gives an account in which the neutral region close to the sun is explained as being Brewster's point; the existence of negative polarization below it disproves this.

Nothing has been said hitherto as to the variation of stage corresponding to the variation of the zenith-distance of the observed point in the sky.   We have no polarimeter measures, in Tyndall's experiments, of the variation of the polarization of light normally emitted with the variation of the neutral angle. In the observations of the sky, however, we have some material, though not very direct, bearing on this point.   Observations of the distance of the neutral points from sun and antisolar point, at varying altitudes, occur in Brewster, and also observations of

maximum and of horizontal maximum polarization. From some few of these the general conclusion may be drawn, that, in

Fig. III.

Arago'
Point.

Babinet's
Point.

Fig. IV.

Babinet's
Point.

Sun.
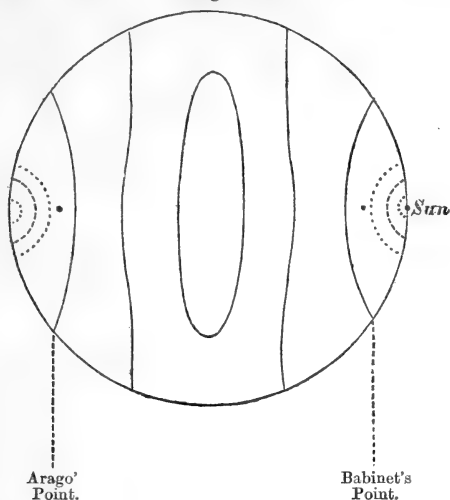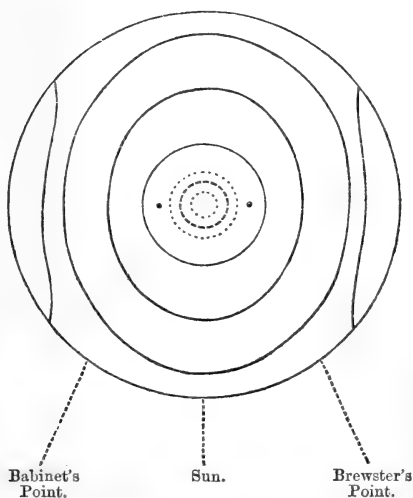
Brewster's
Point.

the best condition of the sky, when the point observed is in the zenith the neutral angle is small; *e. g.* if the sun be high up, Babinet's point and Brewster's point move up near to it. Brewster infers that, if the sun were actually in the zenith, the neutral points would coincide with it; but although he

states this as a fact, it is clearly only an inference. If he had been led to connect the evanescence of the neutral angle with perfect polarization in a direction normal to the sun, he would hardly have said so. As a general rule, the zenith maximum polarization is about 30°, and the horizon maximum less, the zenith neutral angle small, and the horizon neutral angle about 20°. These data define pretty nearly the relative stages.

To gain some light on the general relation between the maximum polarization normal to beam and the neutral angle, I formed a pair of corresponding values, by taking from Brewster's observations eight sets of pairs of values, of four or five pairs each, all from good-days' observations. The polarizations taken were the horizon maxima. Although observed in a direction at right angles to the neutral point, yet being about the same altitude (for Brewster generally observed the neutral point immediately after it rose), in the mean of still days the air might be expected to be in the same condition in the two places. The resulting pair of values was :—

Maximum polarization in horizon.	Altitude of Arago's point above antisolar point.
25°	20° 20′

The observations of Arago's Point are far superior to those of the other points, the observations of the latter being much more difficult; I therefore confined myself to the former.

It was then natural to try and formulate the general relation between the polarization and the neutral angle, bearing in mind the terminal conditions, and requiring a somewhat close agreement with the above normal value. After numerous trials I hit upon the formula

$$\sin \alpha = \tan (45° - R),$$

where α is the neutral angle, and $45° - R$ the polarimeter angle for normal emission.

This satisfies the terminal conditions, making $\alpha = 0$ when $R = 45°$ (Stage I.), and $\alpha = 90°$ when $R = 0$ (end of Stage II.); and for $R = 25°$ we have $\alpha = 21° 20′$, which is just a degree from the value 20° 20′ of the observations.

To give some idea of the character of the observations the following Table is subjoined; it contains the eight means employed above, each number being the mean of such observations on one day as have seemed to me to have been taken under comparable circumstances.

R. Zenith maximum.	R. Horizon maximum.	Altitude of Arago above antisolar point.
29° 50′	28° 50′	21° 0′
29 0	29 0	19 0
28 0	20 30	19 20
29 0	27 0	20 30
27 30	25 30	21 40
25 0	22 30	20 0
22 30	19 30	20 0
27 0	27 30	21 0

Considering the roughness of the methods of observation, and the considerable irregularities, the deviation of one degree from the result will not affect the representation of the general course of the observations. I might extract any quantity of the observations; but it seems better to leave them to be referred to by any one interested in the subject. I do not believe that it is possible to represent their general course, as far as this point is concerned, better than by the above formula. More numerous and better observations may be hoped for at some future time; with what we have I do not think we can get any further.

The observation of the polarization of normally emitted light with the polarimeter, simultaneously with the observation of the neutral angle, in experiments such as Tyndall's on smoke, will furnish results the comparison of which with the above formula, or with any subsequent formula or explanation, will be of interest.

Recurring to the transformation of Brewster's formula, and putting R_0 for the maximum normal polarization and α for the neutral angle corresponding to the stage of any portion of the sky observed, we have, applying the above formula, $R_0 = 45° - \tan^{-1} \sin \alpha$; and substituting this value for the zenith-constant, we get, for any inclination θ,

$$R = \frac{45° - \tan^{-1} \sin \alpha}{\cos^2 \alpha} \sqrt[n]{\sin \theta} \cdot \sin(\theta - \alpha) \sin(\theta + \alpha).$$

I do not mean to say that this extended formula rests on any detailed evidence; but it represents the general course of the values, and indicates the direction in which observations are required, or, perhaps, rather experiments.

I have already adverted to the observations of Rubenson as supplying values of the variation of maximum polarization from zenith to horizon. The rough rule I deduced from some of his sets is, expressed in Brewster's measure, subtract from the maximum zenith polarization one twelfth of the zenith-distance.

Rubenson's observations are much complicated by the variation of the time of day, so that there is a difficulty in disentangling the effects; and Brewster's impression seems to have been in favour of a smaller correction. Some of his observations, however, favour a larger one than he employs. The point will repay investigation. Experiments made in Tyndall's manner can hardly deal with this point. In fact there can be no doubt that when a beam is observed obliquely, the line of vision travelling through a greater thickness of the beam than in the normal position, the stage of the phenomenon is slightly more advanced than in the normal position. I have endeavoured to imagine an arrangement of the experiments which should eliminate this complication[*]. The case of the sky alone can at present supply information on this point. There we observe in the zenith under the same conditions as in the position normal to the experimental beam; and in the horizon, at right angles to the sun, we have the observation through an increased thickness, but still in a direction normal to the incident light. This advancement of the stage when the beam is regarded obliquely, probably accounts in some measure for the fact that the direction of emission for maximum negative polarization always forms so small an angle with the incident beam. The smaller this angle is, the greater will it be necessary to take n in the term $\sqrt[n]{\sin \theta}$ in the above empirical formulæ.

The theory of these phenomena has been treated by Lord Rayleigh (see Phil. Mag. vol. xli. pp. 107, 274, 447, 519). The few remarks I propose to make on the general theory are offered with diffidence. At present I shall allude to only two points.

The theory which has seemed most satisfactory rests on the assumption that the effect of matter on the æther is simply to load it, *i. e.* to increase the inertia. Lord Rayleigh has employed an illustration (p. 521), in which he shows, from the analogy of the motion of an ellipsoid in a fluid, that there is nothing absurd in the idea of an inertia varying with the direction of motion (p. 523). But the physical hypothesis on which the theory then proposed is built up is not very precisely defined. Now it seems to me that there is a great difficulty in employing the hypothesis of the loading of the æther unless the mode in which the loading is originated is defined. I take it that the

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[*] This could be done by employing a beam whose section is of the form of a long and narrow rectangle, *i. e.* a beam forming a thin sheet of light. By varying the inclination of the line of vision to the sheet in a plane at right angles to the lines of light, the effect here alluded to might be observed. But it would be difficult to arrange such a beam unless, perhaps, with direct sunlight.

object of physical theories of this kind is to account for the observed facts by an explanation which employs only the ordinary principles of mechanics ; so that any communication of energy between the æther and material particles must be effected by the action and reaction of pressures arising out of relative motion ; otherwise the explanation is of the kind *ignotum per ignotius.* Now if we make in so many words the hypothesis that the density of the æther is the same within as without all bodies without further restriction, the effect is to suppose that the ultimate particles of matter and the æther mutually interpenetrate each other, *i. e.* that they can occupy the same space without interfering with each other. This is, as far as I can see, not a physically conceivable hypothesis. And when we turn to the mutual action, we have to make some additional hypothesis and introduce forces originating in a different manner from the pressures which act between any two bodies not mutually interpenetrable. Any hypothesis, for instance, which supposes mutual interpenetration excludes such analogies as that of Lord Rayleigh above referred to, in which the ellipsoid and surrounding fluid are certainly not supposed interpenetrable. It seems to be that the hypothesis needed is something of this sort—that material bodies are built up like a complex network in space, the interstices of which are considerable as compared with the space occupied by the material rods or points of which they consist. The relation to the æther may be illustrated by supposing such a network built up of rods like very fine needles ; the whole could then be pushed with ease into a mass of jelly. The jelly would separate before and close again after the rods as they passed through it. The separation of the jelly by the rods is not inconsistent with its being substantially incompressible ; but the result would be that the æther would be parted on each side of a solid body moving through it to an extent depending on the ratio of the space occupied by the matter to that occupied by the æther within the body. It seems to me that some such hypothesis as this is necessary for any explanation of the facts of radiation and absorption ; and so far as I can see, it would not be incompatible with the application of Green's theory or of that proposed by Lord Rayleigh. It is fully consistent with Lord Rayleigh's treatment of the problem of the first stage of Tyndall's clouds. It also seems to me to afford an explanation of the phenomenon of residual blue.

It is only necessary to admit that the size of the particles in the first stage is small, not only with respect to the wave-lengths, but also with respect to the amplitudes of the incident light. Consider then the stage at which the mean diameters of the particles are equal to the amplitudes of the incident light. (In

all cases in which residual blue has been observed, the diffracting matter has been in the state of vapour, and the particles would be spherical.) Now the relative oscillation of a sphere through a space equal to its own diameter in a medium not entirely penetrable by it must give rise to a displacement of the medium, some part of which will be at right angles to the direction of oscillation of the sphere ; and it is evident that the principal term of this displacement will have a periodic time equal to half that of the sphere itself. (The relation has some resemblance to that between the vibrations of a tuning-fork and of a string attached to it at right angles to its length.) Under these circumstances a certain proportion of the incident red and ultra-red rays would give rise to diffracted light of halved periodic time, *i. e.* to ultra-violet and violet negatively polarized. This transformation, however, would soon disappear as the particles increased further in size.

The second point I propose to allude to is as follows. It appears to me that it is impossible that the inertia of the material particles can in general be the source of the diminished velocity of light within transparent bodies. For if the inertia of the particles acts in this way, they must, whether they oscillate themselves or not, absorb energy in the manner of a friction-brake ; it is impossible that they should act as loads to the æther, and not themselves absorb energy ; and the energy so absorbed must be converted into heat. It should therefore always be the case that the greater refractive index corresponds to the greater absorption, if it be true that the inertia of the particles is always the source of the refraction. But this is not the case. For instance, in glass the absorption of rays of high refrangibility is very small ; whereas the heat-rays of low refrangibility are almost entirely absorbed. Other similar instances will occur to every one.

A few words may be said as to the formula

$$\sin \alpha = \tan (45^\circ - R).$$

α is the neutral angle, $\sin 2R$ the polarization of light emitted normally.

Let A be the mean amplitude of the diffracted light in the plane normal to the beam, B parallel to the axis of the beam. In the first stage $B = 0$; in the second B increases up to $B = A$, which marks the termination of the second stage.

The normal polarization is $\dfrac{A^2 - B^2}{A^2 + B^2} = \cos 2\phi$, where $\dfrac{B}{A} = \tan \phi$; and if we define R by the condition that polarization $= \sin 2R$, we have at once $2\phi = 90^\circ - 2R$, or $\phi = 45^\circ - R$. (Brewster's ϕ is

$45° + R$; with this it is only necessary to change the sign of $\cos 2\phi$.)

Then, admitting the formula $\sin \alpha = \tan \phi$, we have

$$\sin \alpha = \frac{B}{A}.$$

We may put this in the form $A^2 = A^2 \cos^2 \alpha + B^2$. Now, if we look at a point on the beam in a direction enclosing an angle θ with the beam in a horizontal plane, we have

Vertical component . . . . $= A,$
Component of the horizontal A perpendicular
   to the line of vision . . . . $= A \cos \theta,$
Component of B . . . . $= B \sin \theta.$

But we cannot assume that the A and B seen from any angle θ are always the same. In the first stage we may assume this with great probability ; in the second it is easy to see that there could be no neutral angle if A and B were always the same ; for then we should have as the condition for the neutral angle

$$A^2 = A^2 \cos^2 \theta + B^2 \sin^2 \theta,$$

which can only be satisfied by $\theta = 0$. Let, therefore, the changed values of A and B in the horizontal plane for angle θ be represented by A', B' ; then the condition for the neutral angle becomes

$$A^2 = A'^2 \cos^2 \theta + B'^2 \sin^2 \theta.$$

Comparing this with the empirical formula

$$A^2 = A^2 \cos^2 + B^2, \text{ if } \theta = \alpha,$$

we get

$$B' \sin \theta = B, \quad A = A'.$$

Now, if we took B to be a measure of the stage and assumed (what we know is not true) that the stage is so advanced by the increasing thickness of the beam that B' is proportional to the thickness observed, then (since the thickness observed at inclination θ is inversely as $\sin \theta$) we might put $B' = \dfrac{B}{\sin \theta}$, which would reduce all the experimental phenomena to one simple law. Regarding the experiments alone, there is nothing to show that this explanation is not correct, except the probability, derived from the very perfect theory of the first stage, that the size of the particles, and not the thickness of the observed stratum, is connected with the advancement of the stage. But the sky-observations are conclusive, and show at once that this explanation will not hold ; for there, as I have already said, we observe directly the change of stage due to the

variation of thickness from zenith to the horizon, and it is quite insufficient to admit of this explanation.

If we admit that A^2 and $A^2 \cos^2 \theta + B^2$ represent the vertical and horizontal components in the case in which θ is not the neutral angle, we have for the polarization

$$\frac{A^2 \sin^2 \theta - B^2}{A^2(1 + \cos^2 \theta) + B^2}, \quad \text{or} \quad \frac{\sin^2 \theta - \sin^2 \alpha}{1 + \cos^2 \theta + \sin^2 \alpha}.$$

Now it is true that this assumption is unsupported by evidence, except that the expressions are true in the case $\theta = \alpha$. But they derive from this a certain probability, and may be useful as giving a simpler empirical formula than the one derived from Brewster's expression. In one point the result is curious. That is, that this expression agrees with Tyndall's observations and with Brewster's original and modified formulæ (if the correction $\sqrt[n]{\sin \theta}$ introduced by me be omitted) in making the negative polarization increase continually up to the smallest inclinations to the beam in cases where there is a neutral angle. (Of course, if there is no neutral angle, either we are in Stage I., and $B=0$, or we are in Stage III., and $B > A$.) The Brewster formulæ, as well as the one last stated, must necessarily be wrong in attributing the greatest value of negative polarization to a point of view ultimately coinciding with the beam. But the coincidence of the indications of both with Tyndall's observations, as far as they go, is curious. For the present I am inclined to keep both formulæ as probable empirical representations, affecting them with the factor $\sqrt[n]{\sin \theta}$, the purpose of which has been already explained.

The methods employed in reducing the indications of the polarimeter to absolute values have been based on two different principles. I shall discuss these briefly, and then indicate a third method, which seems to me preferable to either. The first method is that of Brewster; it is similar in principle to that developed in a paper by Professor Adams (Phil. Mag. vol. xli. p. 205). The methods differ only in that Brewster's formula appears to be based on experiment, Adams's on theory.

The form of the polarimeter is always the same. It consists of:—an analyzing eyepiece, a Nicol or tourmaline; a test-object, which, with the eyepiece, indicates the existence of polarized light and the direction of polarization; and a bundle of glass plates placed in front of the test-object, by which the polarization of partially polarized light incident on the instrument can be compensated. The eyepiece and test-object together constitute a polariscope. The problem is, from the observed inclination of the bundle of glass plates, to deduce the value of the

polarization of the incident light which is neutralized by the bundle.

The formula of Brewster appears to be founded on experiment. It was first given in Phil. Trans. vol. cxx. (1830) pp. 136, 141. The formula is $\cot \phi = \cos^{2n} (i - i')$, where $\phi = R + 45°$, so that $\cot \phi = \tan (45° - R)$; i is the angle of incidence, i' the angle of refraction, and n the number of plates. It would appear that in Brewster's paper (Edinb. Trans. vol. xxiii. p. 233) the formula is erroneously quoted, the 2 in the index being omitted. Brewster calculated by this formula the polarization corresponding to various inclinations for the glasses he employed.

In the paper of Professor Adams above referred to, Tables are given for a few inclinations for four plates and for two values of μ, and general expressions are given for any case. The mode of statement is more direct than that of Brewster. It is, however, obvious that this method must always be subject to considerable uncertainty. It is impossible, by any means with which I am acquainted, to verify the refractive index of the glass of which the plates are composed; and looking to the mode in which the plates are formed, the assumption even that the glass of all the plates has the same refractive index must be of doubtful admissibility; while the assumption that it is the same as that of the sample of which the refractive index has been determined, seems to introduce a still more considerable element of uncertainty. At the same time there is no doubt that a first approximation to the truth may be got in this way. The variation in the colour of the light observed would also seem to be a source of error.

The second method is that employed by Rubenson, which has been already alluded to. The polarization corresponding to the different inclinations of the glass bundle is determined by a process of graduation, which is somewhat as follows. Polarized light issues from a Nicol's prism, and falls upon a quarter-wave plate, the direction of the axis of which is parallel or perpendicular to the axis on which the glass bundle turns, the bundle being arranged so as to compensate the resulting polarization. If, then, the angle between the plane of polarization of the Nicol and the axis of the plate be ϕ, the intensities of the components of the resulting elliptically polarized light along and at right angles to the axis are $A^2 \sin^2 \phi$ and $A^2 \cos^2 \phi$ respectively, and, as before observed, the polarization is $\cos 2\phi$.

The objections to this proceeding are :—first, that we depend on the accuracy of the quarter-wave plate, where we may have error of workmanship, and must have error arising from variation of colour; secondly, that we introduce Malus's law into our determinations; and strictly speaking we cannot trust the accuracy of this law beyond the point to which it has been car-

ried by actual measures; and, again, we make the assumption that the polarization of elliptically polarized light is determined by the same laws as the polarization of partially polarized light—an assumption which it would seem desirable to avoid in determinations of an absolute character. Now the measure of the ratio in which polarized light and common light are mixed can, theoretically at least, be performed by a direct process which does not involve any intermediate law, whether theoretical or empirical. It is only necessary to provide two apertures which shall admit polarized and common light in known proportions, to superpose these lights, and determine by observation the inclination of the glass bundle employed which will neutralize the polarization. I have not constructed any such arrangement; I have been unable, in fact, as yet, to devote any resources to the practical part of the question, in consequence of the large demands made on me by the construction of instruments in another branch of inquiry; but I propose to describe the outline of an arrangement for this purpose which might be constructed.

A simple lens of some feet focal distance is cut in half and mounted in the manner of a heliometer; the micrometer arrangement is not required, only that the two images given may be made to coincide and separate at pleasure. In front of each half of the lens a screen is mounted, with an aperture the area of which can be regulated by a micrometer-screw. A Nicol's prism large enough to supply the whole of one of the apertures with polarized light, is placed in front of it. A simple lens, or Ramsden's eyepiece, may be employed to view the images. The sun may be viewed through a dark glass or any ordinary source of unpolarized light directly. The images are separated, their brightness compared, and the movable apertures adjusted until the brightness of the two images is the same. This furnishes the values of the two apertures which admit the same amount of light when the Nicol is in front of one. By the micrometer arrangement either area can be altered in a known proportion, taking care always to keep the aperture behind the Nicol a good deal smaller than that of the prism. The two images are then superposed; and a single image is thus formed, in which polarized and common light are mixed in known proportions. We have only to observe the inclination of the glass bundle of the polarimeter required to compensate this polarization for varied proportions of common and polarized light; and the instrument will be graduated independently of instrumental data, except such as can be verified at any time and independently of any hypothesis.

The effectiveness of a polarimeter for the purpose of observing values of polarization such as occur in the sky at right angles to the sun is determined by different conditions from those required for the observation of neutral points, and of negative polarization such as occurs in the sky. For the former purpose a very sensitive test-object is not needed, but it is required to know the values of the inclinations of the glass bundle. For the latter purpose, on the other hand, it is necessary to have a test-object of the utmost delicacy; and the glass bundle interferes so much with the observations, that it seems generally best to discard it, and employ simply the most delicate polariscope attainable. I have constructed a polarimeter out of the materials I had at hand: the analyzer is a small Nicol; the test-object a biquartz, formed of two pieces of right- and left-handed quartz which overlap in the middle thus—

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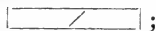
and the glass bundle consists of ten thin plates. If the values of the inclinations of the bundle were known, the instrument would be admirably adapted for the measure of maximum polarizations, the overlapping portion of the biquartz forming a test-object of amply sufficient delicacy. But with this instrument, observing certainly for the most part under very unfavourable circumstances, I have never yet been able to distinguish the neutral points or negative polarization in the sky.

Brewster's test-object appears to have been much more sensitive. The polariscope was presented to him by Babinet. To give the appearance shown in the drawing of the neutral line at p. 213 (Edinb. Trans. vol. xxiii.), it must have been of great delicacy. It consisted of the well-known arrangement of two wedges of rock-crystal with the axes crossed.

If any progress is to be made in the subject, it is to be desired that both observations and experiments should be instituted. The Radcliffe observer (Mr. Main) has offered to assist in bringing the subject before astronomers. And we might learn much if Professor Tyndall were to resume, or some other physicist to take up, the experimental inquiry.

LX. *On Unitation.*—V. *Some of the Applications and Developments of the General Formula. By* W. H. WALENN, *Mem. Phys. Soc.*

11. THE most general statement of the principal formula of unitation is, that if a given number be expressed in terms of its digits and of the powers of 10, the formula

$$(10-\delta)^{n-1}a + (10-\delta)^{n-2}b + (10-\delta)^{n-3}c + \ldots + (10-\delta)^2 s$$
$$+ (10-\delta)^1 t + (10-\delta)^0 u + (10-\delta)^{-1}v + (10-\delta)^{-2}w$$
$$+ \ldots + (10-\delta)^{-m}z$$

has the same remainder to δ as the given number has, n being the number of integer digits in the given number, and m being the number of digits on the right-hand side of the decimal point.

The simplest form of the theorem from which the above expansion can be made is the statement as put forth in the first paper on the subject*, namely :—" that if t be the tens' and u the units' digit of a two-figure number, and δ be any integer less than 10, then

$$(10-\delta)t + u$$

has the same remainder to δ as $10t + u$." The following proof of this simplest statement is also applicable to the expanded forms of the theorem. By multiplication,

$$(10-\delta)t + u = 10t - \delta t + u = (10t + u) - \delta t ;$$

but the latter expression only differs from $(10t + u)$ by an exact number of times δ. Further, if the digits be s, t, u, the same is true of $(10-\delta)^2 s + (10-\delta)t + u$, and so on ; for each time 10 occurs as a factor in any term, it must be treated in the way above indicated.

12. Another proof of the general theorem may be given, by induction. It is well known that the addition of the two-figure digits (excepting 99) in the nine-times multiplication table always gives nine. If a method of obtaining a similar property in other numerals were discovered, the remainders to division by the said numerals would be found without using the operation of division ; for if the process left any unit, and if the unit were not already less than the divisor, it would be reducible to a quantity less than the divisor, and therefore to the remainder required, by simple subtraction. In the case of the nine-times table, the following are instances :—$9 . 3 = 27$, and $2 + 7 = 9$; and $9 . 3 + 4 = 31$, and $3 + 1 = 4$.

To oblige the multiplication table of any digit to constantly yield the digit itself (by a process analogous to casting out the nines), the units will certainly have to be added ; what other operation the remainder of the digits must be subjected to is the problem to be solved. The operation of addition (when it does not involve factors) is clearly not sufficient ; for, as an operation, it admits of remainders that are no function of the divisor, except in the case of the nine-times Table. This consideration, combined with the inspection of, say, the

eight-times Table, shows that it is the multiplier 2 which is wanted for the tens digit, to reduce the series 8, 16, 24, 32, &c. to the form 8, 8, 8, 8, &c.; for (calling the result of the operation U_8x), $U_88=8$, $U_816=2.1+6=8$, $U_824=2.2+4=8$, $U_832=2.3+2=8$, &c. The foregoing points also tend to prove that the operation for the δ-times table (or $U_\delta x$) is clearly one of multiplication by $(10-\delta)$, in respect of the tens' digit; accordingly 3 is the multiplier for the 7-times Table, 4 for the 6-times Table, and so on. On reducing the theorem to general terms from these particular results, it appears that the remainder to a given divisor $(\delta)=(10-\delta)t+u$, if the dividend be $10t+u$. This expression is now in a condition to be further expanded into the formula given in article 11.

13. In this place it may be convenient to elucidate the methods of unitation employed in practice, although this point was alluded to in the first paper on unitation. Practically, to unitate a given number, it is best to commence at the left hand, using the multiplier appropriate to the base of unitation, and adding in the next figure to the right hand, at each step of the operation. As soon as any number higher than 9 arises, the result should be reduced (by a continued repetition of the process) to a single figure, and the operation itself continued until only a single figure results therefrom. For example, U_8313 has the multiplier **2**, and the operation is

$$2.3+1=7; \ 2.7+3=17; \ 2.1+7=9; \ 9-8=1;$$
$$\therefore \ U_8313=1.$$

In this case all figures higher than hundreds do not influence the result. Again, U_7515 has the multiplier 3, and the operation is

$$3.5+1=16; \ 3.1+6=9; \ 9-7=2; \ 3.2+5=11;$$
$$3.1+1=4; \ \therefore \ U_7515=4.$$

In some cases it is simpler to obtain the unitate by dividing the given number by the base of the system.

14. In most arithmetical works, the St. Andrew's cross, employed by Lucas de Burgo and others, is still used to formulate the casting out of the nines. The methods which unitation discloses, all point to operating with the unitates in the equational form. The following examples show this practical point.

Example I. Given

$$x=2349 \times 876=2057724;$$

then

$$U_{11}x=U_{11}(6\times7) \ =9;$$

also

$$U_{11}2057724 \qquad =9,$$

thus checking the multiplication above indicated.

Example II. Given

$$x = \frac{14}{7 \cdot 854} = 1 \cdot 782 + \frac{\cdot 004172}{7 \cdot 854} \; ;$$

then

$$U_9 x = \frac{5}{6} \; ;$$

also

$$U_9 \left(1 \cdot 782 + \frac{\cdot 004172}{7 \cdot 854} \right) = U_9 \left(9 + \frac{5}{6} \right) = \frac{5}{6},$$

thus checking the above division by a direct process.

Example III. Given

$$x = \frac{239406}{12100} = 19 + \frac{9506}{12100} \; ;$$

then

$$U_9 x = \frac{6}{4} = U_9 \left(6 \times \frac{1}{4} \right) = U_9 (6 \times 7) = 6 \; ;$$

also

$$U_9 \left(19 + \frac{9506}{12100} \right) = U_9 \left(1 + \frac{2}{4} \right) = U_9 15 = 6.$$

In this instance advantage is taken of $U_9 \frac{1}{4} = U_9 4^{-1} = 7$ *.

Example IV. Given

$$x = \frac{29137062}{5317} = 5479 + \frac{5219}{5317} \; ;$$

then

$$U_{11} x = \frac{9}{4} = U_{11} (9 \times 3) = U_{11} 27 = 5 \; ;$$

also

$$U_{11} \left(5479 + \frac{5219}{5317} \right) = U_{11} \left(1 + \frac{5}{4} \right) = U_{11} \left(1 + 5 \times \frac{1}{4} \right)$$
$$= U_{11} (1 + 5 \times 3) = U_{11} 16 = 5.$$

Here advantage is taken of $U_{11} \frac{1}{4} = 3$.

In reference to the practical advantage of unitation for checking calculations, it must be borne in mind that the obtainment of unitates and the calculations therewith can be performed mentally with ease. It is more laborious to set down the steps of the unitation, or of the operation with unitates, in order, than to do them mentally.

15. In the eleventh volume of the 'Mechanics' Magazine' a discussion upon a property of numbers ran through nearly the whole volume. The proposition first advanced was that if the digits of a given number were reversed in their order (251 made

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* Phil. Mag. S. 4. vol. xlvi. p. 40.

into 152 for instance), and the lesser number subtracted from the greater, the remainder would be exactly divisible by 9. The statement was then extended to deriving the second number from the first by transposing the figures of the first in any order. The proof of this theorem by unitation presents some interesting points, inasmuch as it shows how this property of numbers may be expanded still further.

If B be the number thus derived from the given number A, then $U_9 A = U_9 B$. For A and B have the same digits, and the order of their addition does not affect the unitate of either of the numbers. Therefore $U_9 A - U_9 B = 0$ or 9; or $\dfrac{A-B}{9}$ ($\dfrac{B-A}{9}$ if B be greater than A) gives no remainder.

Further, the derived number may have either noughts, or nines, or multiples of nine in addition to or in defect of those in the original number, and the theorem will still be true. For example,

$$105063259723 - 153362752 = 104909896971, \text{ and}$$
$$U_9(7-7) = U(16-7) = 9,$$

which is the unitate of 104909896971.

16. The derived number may be formed from the given number by other methods, so that the remainder to the subtraction may have other properties ; unitation suggests these properties and furnishes proofs of the same. If dots or marks be made at equal intervals over the digits of the given number, or a part of the given number, and (having regard to the relative position of the marked digits in the number) if the marked digits be transposed among themselves, the remainder will be exactly divisible by some number composed entirely of ones, as 1111 for instance, also by some other number composed entirely of nines, as 9999 for instance, the number of ones or of nines in the divisor being $a+1$, if a be the number of intervals. If the marks be placed over the 1st, 3rd, 5th, &c. digits (leaving one interval), the remainder will be divisible by 99 and 11, if over the 1st, 5th, 9th, 13th, &c. digits (leaving three intervals), the divisors will be 1111 and 9999, and so on.

For if the unitation formula (see article 11) be arranged so as to assume the form

$$(10-\delta)^{3.3}l + (10-\delta)^{2.3}o + (10-\delta)^3 r + u,$$

and 111 substituted therein for δ, each term (when unitated to the base 111) will be found to have $+1$ for its coefficient. In this case the substitution gives negative values to every second term commencing with the second from the right hand ; but the subsequent unitation of these values, taken as positive quanti-

ties, gives -1, showing that each coefficient is $-(-1)=+1$. Other cases may be proved by similar means. When 999 is substituted for δ in the above form, the coefficients, reduced by unitation, are also $=+1$.

The following example illustrates this property in regard to the divisors 111 and 999, the given number being the lesser of the two.

$$5\ 8\ 2\ 1\ 2\ 3\ \overset{\text{\tiny I}}{1}-\overset{\text{\tiny I}}{1}\ \overset{\text{\tiny III}}{2}\ \overset{\text{\tiny II}}{3}\ \overset{\text{\tiny I}}{5}\ \overset{\text{\tiny III}}{8}\ \overset{\text{\tiny II}}{2}\ \overset{\text{\tiny I}}{1}=4585410.$$

17. Some years ago Mr. Henry Butter, of educational celebrity, showed the writer a property of the decimal equivalent to the reciprocal of 19. The right-hand figure of the recurring period of this decimal is 1; and the whole period may be constructed from the right-hand end by using the multiplier 2 to obtain the next figure, towards the left, from the previous one, adding in the tens' digit that may be carried from the previous product. The decimal equivalent thus produced is

$$\tfrac{1}{19}=\cdot0\dot{5}263157894736842\dot{1}.$$

Considering this property with reference to unitation, and believing the case not to be an isolated one, the writer has ascertained that certain decimal fractions can be formed from the right-hand end by means of the coefficients of certain systems of unitates.

Those decimal equivalents of reciprocals which have the terminal figure of their period equal to 1 (the corresponding reciprocal being of the form $\dfrac{1}{10a-1}$) are related to the coefficients of unitates, so that the multiplier (a) necessary for their formation is dependent upon the $(10-a)$ system of unitates. The following example shows this point.

Take $\tfrac{1}{39}=\cdot0\dot{2}564\dot{1}.$ Let c_1, c_2, c_3, c_4, &c. be the coefficients of the function U_6x (the series of powers of 4), then

$$
\begin{aligned}
1 &= c_1\\
40 &= 10c_2\\
1600 &= 10^2c_3\\
64000 &= 10^3c_4\\
2\ |\ 560000 &= 10^4c_5\\
102\ |\ 400000 &= 10^5c_6\\
\hline
025641
\end{aligned}
$$

and the recurring figures of $\tfrac{1}{39}$ are given by the six right-hand figures of the formula $10^5c_6+10^4c_5+10^3c_4+10^2c_3+10c_2+c$.

Since $\tfrac{1}{7}=\tfrac{7}{49}$, $\tfrac{1}{17}=\tfrac{7}{119}$, &c., this law is extensible to reciprocals of the form $\dfrac{1}{10n-3}$; those of the forms $\dfrac{1}{10n-}$ and $\dfrac{1}{10n-9}$

are similarly dealt with. It is worthy of notice that in the Rev. George Peacock's celebrated treatise on Arithmetic in the *Encyclopædia Metropolitana*, p. 499, the decimal equivalent for $\frac{1}{17}$ is given $= \cdot 05882352941$ (having the portion $\cdot 05882$ finite, although 17 is a prime number), whereas the process above indicated gave the value as $\frac{1}{17} = \cdot 0588235294117647$. The latter value has a repeating period of sixteen figures, and is found to be correct.

This theorem applies to all recurring decimals.

74 Brecknock Road, N.
    October 1875.

[To be continued.]

LXI. *Spectral-Analytical Researches.* By R. Bunsen [*].

[With Three Plates.]

[Continued from p. 430.]

II. Spark-spectra, Flame-spectra, and Absorption-spectra of the Elements.

THE appearance presented in the spectroscope by a continuous spectrum is chiefly dependent upon the breadth of the slit. Homogeneous light appears as a sharply defined line of one colour, whose apparent *breadth* increases in proportion with the breadth of the slit. If, however, the rays of light proceed from several neighbouring sources, then they appear, arranged in the order of their colours, as a more or less broad band, whose apparent extension is not proportional to the breadth of the slit, but to this breadth plus a constant. Such bands therefore increase in a less ratio than does the breadth of the slit.

The influence exercised upon the appearance of these bands by the gradation and maxima of light is also to a great degree conditioned by the breadth of the slit, because the images produced by the widening of the slit become superimposed upon those which are already present. Groups of new lines, which entirely change the habitus of the original spectrum, may therefore become visible by gradually narrowing the slit: thus in the spark-spectrum of yttrium the peculiar bands appearing in the red are resolved, on narrowing the slit, into a great number of sharply defined lines, which, from the peculiarity of their position and intensity, serve as tests for the presence of yttrium.

Next to the breadth of slit the intensity of the source of light exercises the greatest influence upon the formation of the spectrum. Vapours, when exposed to the high temperature of the Leyden-jar spark, often exhibit a great number of lines, which either disappear or become too faint for recognition at the lower temperature of the simple spark or non-luminous gas-flame. With increased intensity of light continuous spectra are often produced, in consequence of which many of the feebler lines, which would otherwise be perfectly apparent by contrast, more or less escape recognition. The fact, first announced by Kirchhoff, that the relative intensity of the individual lines does not alter proportionately with increase in temperature of the source of light, explains the other fact that these lines, which are relatively feeblest in the flame-spectrum, sometimes become the most marked in the spark-spectrum, as is especially the case with the spectrum of lithium. On the other hand, the apparently contradictory fact has been noticed, that the flame-spectrum of several substances is much sharper and richer in lines than the spark-spectrum; for instance, the lines which are so characteristic in the flame-spectra of cæsium and of rubidium disappear, the former entirely, the latter to a less degree, in the spark-spectra of the same elements. This, however, is easily explained when we consider that the light proceeding to the slit from the glaring mass of flame (which is of larger dimensions although not of so high temperature as the spark) is of greater intensity than that which proceeds from the train of sparks, where the quantity of gas is very limited although of extremely high temperature. Those substances whose spectra become apparent at low temperatures are best examined in the gas-flame, and not with the spark. The principal spectra belonging to this class are those of the alkalies, of the alkaline earths, of iridium, thallium, and some others. Such spectra are most perfectly produced by bringing the little beads to be tested supported on the unlooped end of a very fine platinum wire, into the fusion-zone of a non-luminous gas-flame.

From these considerations it follows that the practical application of spectra necessitates the possibility of a variation in the spark-temperature within not too wide limits, and the choice of a breadth of slit, to be maintained throughout the observation, by which a sufficient separation of the characteristic lines shall be secured without such a diminution of the light as may prevent a clear perception of the spectrum. In laboratories, where *one* prism only is commonly used in the observation of spark-spectra, these conditions are most easily fulfilled by narrowing the slit until the two characteristic bands of yttrium chloride in the red are resolved into a distinctly perceptible mass

of lines, and when the induction-current used for the production of Leyden-jar sparks is of such a strength as to cause the formation of sparks between thick pointed platinum wires from 1 to 2 centimetres apart.

In our first memoir on spectral analysis we stated that the spectra of the elements are by no means always identical with the spectra of their compounds. Such an identity, it is true, is often noticed, especially in the case of those elements which, when in the state of glowing vapour, give out homogeneous light; yet it appears to me that the question as to the apparent or real nature of this identity has not as yet been decided, in any instance, with strict scientific accuracy.

Although sodium vapour, as I have before pointed out, exhibits dark lines at the temperature of boiling mercury (much below a red heat), and although these lines are observed, reversed as bright lines, in the glowing vapour of all volatile sodium compounds, nevertheless we cannot as yet finally decide whether these similar spectra are derived from the element as well as from its compounds, or whether they are derived from the element alone, set free by the decomposition of the compound bodies. How this question may be solved is of little importance in the practical application of spectral analysis. It concerns us merely to remember that the spectrum of an element is not always independent of the compound in which it occurs, and that it therefore appears necessary to choose certain fixed compounds for the recognition of the various elements.

The chlorides were employed in these researches, as in our first investigation, on account of their volatility, and of the ease with which they may be prepared. The order in which the spectra appear when a mixture of chlorides is vaporized by the flame or spark, depends upon the relative quantities and volatility of the chlorides. If a substance, volatile only at high temperatures and giving a well-marked line-spectrum, be mixed with increasing proportions of another, volatile at low temperatures but giving no spectrum, the spectrum of the former is gradually rendered less marked until at last it can scarcely be recognized. If both substances give line-spectra, the spectrum of the more volatile is generally first observed; and it is only when this has become very faint by repeated moistening with hydrochloric acid and strong heating, that the spectrum of the more difficultly volatile substance comes into view. This phenomenon finds its explanation in the circumstance that the temperature of the heated mixture is always equal to the volatilization temperature of the least volatile of the substances contained in it, so that those substances which are volatile at a higher temperature cannot as yet be vaporized.

The carbon points of the spark-apparatus, when dry, or when moistened with hydrochloric acid, gave no spectrum, provided they had been thoroughly cleaned by the method already described; this was shown by the fact that the sparks passing between them in an atmosphere of hydrogen showed only the less characteristic lines of the latter.

The sparks which passed in the air during the spectral observations showed therefore only the air-lines of oxygen, nitrogen, and hydrogen.

This air-spectrum is given in the first line of each of the spectral Tables (Plates III.–V.), so that no confusion may be made between it and the lines which show the presence of the individual elements.

In the Plates f indicates the flame-spectrum, e the electric-spark spectrum, a the absorption-spectrum.

1. *Spectra of the Elements of the Alkali Group.*

The pure chlorides, whose spectra are shown in Plates III. and IV., nos. 1 to 6, were prepared as follows :—

Sodium chloride, prepared from hydrochloric acid and sodium carbonate, was purified by washing with concentrated hydrochloric acid and repeated recrystallization.

Pure potassium chloride was prepared by strongly heating potassium chlorate which had been six times recrystallized.

The material with which I determined the atomic weights of cæsium and rubidium, and which was prepared in a state of great purity by a method described elsewhere, was used as the source of the salts of these metals.

Thallium chloride, precipitated by hydrochloric acid from the sulphate, was purified by repeated crystallization.

The exceedingly pure preparation of lithium with which the atomic weight of that metal was determined by Mr. Diehl in my laboratory, served for the production of the lithium-spectrum.

The whole of these chlorides were easily and completely volatilized when supported on platinum wire in the non-luminous gas-flame. Test quantities of them, prepared by fractional crystallization, showed not the smallest trace of impurities when tested in the spectrum-apparatus. A glance at Plate III. is sufficient to convince one that all of the members of the alkali group may be recognized by their flame-spectra much more easily than by their spark-spectra. Not a trace of the potassium-chloride lines appears in the spark-spectrum; those of rubidium and cæsium chloride are very faint; while, on the other hand, the flame-spectra of these substances are produced with the greatest clearness and beauty.

The elements of the alkali group are therefore always to be recognized by means of their flame-spectra.

If the whole of the six members of this group be present together, the recognition of lithium, thallium, and sodium is so little influenced by the presence of the others, that quantities of these elements which escape detection by any other means are rendered evident with ease.

The detection of traces of the other three elements when they are mixed with these, as is usually the case in nature, is not, however, so easy a matter, because in the presence of potassium and sodium continuous spectra appear, which greatly destroy the visibility of the other lines.

The following process is adopted in order to remove these continuous spectra—so far, at least, as to enable one to detect with certainty the smallest traces of potassium, cæsium, and rubidium when present together, which can be recognized in no other way.

The chlorides are precipitated with platinum chloride as double salts: the precipitation must take place in a cold concentrated solution, in order that the double salts may be thrown down in a finely divided and, as far as possible, non-crystalline condition. The precipitate is boiled in a platinum vessel, 20 or 30 times, with a little water, which is each time removed by decantation, a small quantity being tested at intervals in the spectrum-apparatus. For this purpose a few milligrammes are brought on to a little piece of moistened filter-paper, which is then surrounded by a very fine platinum wire and exposed to the upper oxidation zone of the non-luminous flame until the paper is completely burned away; the salt is then brought into the fusion zone of the flame placed before the slit of the spectroscope. After the first treatment with boiling water, the potassium-line generally appears beside those of lithium and sodium; as the boiling continues the cæsium and rubidium-lines become gradually more and more apparent. Small traces of thallium, which are often present in mineral waters, begin to make their appearance only towards the end of the washing with boiling water.

2. *Spectra of the Elements of the Alkaline-Earth Group.*

Nos. 7 to 10, Plates IV. and V., show the spectra of the chlorides of calcium, strontium, barium, and magnesium.

Sulphate of magnesium, purified from lime and iron by repeated crystallization, served as the material from which magnesium chloride was prepared.

Barium and strontium chlorides, after repeated treatment with hot alcohol, were purified by crystallization.

Calcium chloride, prepared from limestone free from iron, magnesia, and manganese, was crystallized from absolute alcohol.

The characteristic line of magnesium (Plate V. no. 10), corresponding with Fraunhofer's line *b*, appears only in the spark-spectrum, where, however, it is situated so near to another line (75) that it is with difficulty distinguished from it.

In order therefore to recognize magnesium, it is necessary to substitute hydrogen or coal-gas for the air in which the spark usually passes. The little apparatus represented in fig. 5, Plate II. serves for this purpose. The glass vessel A is furnished with a good caoutchouc stopper, through two holes in which pass platinum wires connected with the carbon-points *a*, *β*. These wires are fused into small mercury cups. The glass tubes *b* and *c* pass through two other holes in the stopper; *b* is connected with a Döbereiner's hydrogen-generating apparatus; *c* carries off the replaced air, and is stopped (when the air has all been driven out of A) by an india-rubber cap. The fine wire *f* conducts the current to the mercury-cup, whence it passes by the small platinum connexion to the carbon point, and so returns by the other wire *g* to the battery.

In order to detect small traces of strontia and baryta when these occur (as is often the case in mineral waters and rocks) mixed with a large amount of lime, the bases are converted into nitrates and digested with small quantities of absolute alcohol. The small quantities of strontium and barium nitrates which remain, or which precipitate after some hours, are collected upon a very small filter and washed with alcohol; the filter is then surrounded by an exceedingly fine platinum wire, and burned in the upper oxidation zone; the ash is treated with a drop of hydrochloric acid, which is applied by means of a capillary tube; and the chlorides thus formed are examined in the flame-spectrum. If, in addition to the sodium-line, the spectrum of strontium is alone visible, the substance on the wire must be repeatedly heated and moistened with hydrochloric acid; the barium-spectrum is then seen, if the minutest trace of that metal be present.

Small traces of calcium and strontium may be detected in barium minerals by digesting the three bases, previously converted into chlorides, with the smallest possible quantity of absolute alcohol and evaporating the liquid. The residue, which is sometimes scarcely visible, is taken up by a slip of filter-paper; the paper is burned on a platinum wire, and the ash, while being repeatedly heated and moistened with hydrochloric acid, is examined in the flame-spectrum.

If it be wished to detect minute traces of calcium and barium in strontium minerals, this may be effected by treating the chlorides with cold and then with hot alcohol. Calcium is found in the first, strontium in the later, and barium in the last washings.

The sulphates of the barium group are not sufficiently volatile to afford flame-spectra. In order to convert these salts into chlorides, the portion under examination is wrapped in several folds of paper, supported on a fine platinum wire and heated in the oxidation-flame until the paper is reduced to ash; the flame is then made luminous, whereby the sulphate is reduced to sulphide; and this salt is converted into chloride by a drop of hydrochloric acid applied by means of a capillary tube.

In these reactions the substance under examination should be repeatedly heated in the oxidation-flame and moistened with hydrochloric acid in order to remove the sodium compounds, the continuous spectra of which often greatly obscure the lines in the spectra of the chlorides.

3. *Spectra of the Elements of the Earth Group.*

The chlorides of aluminium and beryllium do not exhibit lines, either in the flame- or spark-spectra, whereby they may be recognized.

The spectra of the chlorides obtained from the so-called cerite and ytterite earths are mapped in Plate V., nos. 11 to 14.

As the separation of these earths presents very considerable difficulties, and as the methods hitherto employed do not yield altogether satisfactory results, it appears to me to be necessary that I should say something concerning the preparation of the perfectly pure material with which the spectra hereafter described were obtained.

Cerite from Utöe was pulverized and mixed in a Hessian basin with concentrated sulphuric acid in quantity sufficient to fill the basin to one third; the excess of acid was removed by strongly heating the dish; the powdered residue was mixed with water at 0° C., care being taken that no rise of temperature should ensue; the solution was separated by filtration from the solid matter, which was again treated with sulphuric acid, &c. From the solution saturated with sulphuretted hydrogen (in order to remove arsenic, molybdenum, bismuth, copper, and lead), strongly acidified with hydrochloric acid and oxidized by chlorine, the oxalates of cerium, lanthanum, and didymium were precipitated by means of oxalic acid. The oxides, obtained by strongly heating the precipitated oxalates, were dissolved in nitric acid, and the solution was evaporated to the consistence of a syrup over the water-bath. By dissolving the mass after cooling in cold water, and boiling the liquid for some time with water containing 2 cub. centims. of concentrated sulphuric acid per litre, the greater part of the cerium was precipitated as basic sulphate (three litres of the acidified water were required for 250 grammes of cerite).

The precipitate was washed with acidified water, dissolved in a slight excess of dilute sulphuric acid, and the solution reprecipitated by pouring into a litre of boiling water. The solution, precipitation, and washing were repeated two or three times before every impurity was removed from the precipitate. After these processes the greater part of the cerium remains in solution, so that but a few grammes of the pure cerium compound are obtained from 100 grammes of the mixed oxides.

A pure product may be obtained from the various liquids collected during the processes by treating them as described above.

The perfect purity of the substance thus obtained was shown by the following behaviour :—

The pale-yellow hydrated oxide precipitated by caustic potash from the solution of the basic sulphate, when treated with chlorine in a concentrated solution of caustic potash, yielded a deep orange-red oxidation product, without the solution (although saturated with chlorine) taking up the smallest trace of a foreign earth. Cerium oxide, after being strongly heated in the air, is of a pure yellowish white colour, which changes to orange while the oxide is hot. The sulphate does not give the smallest precipitate (thorium oxide) when warmed with sodium dithionate. The oxalate is to a considerable extent soluble in ammonium oxalate solution, but is entirely reprecipitated on dilution with cold water. Small quantities obtained in this way by fractional precipitation, when converted into chlorides, showed one and the same spark-spectra when examined, and gave no indication of the lines peculiar to lanthanum or yttrium. No appearance of the absorption-spectra of didymium or erbium could be obtained with any of the concentrated cerium solutions.

In Pltae V. no. 13 *e*, is shown the spark-spectrum obtained with the chloride of this pure cerium material; the most important lines, which are not, however, very characteristic, are situated at 68, 71, and 79. The preparation of pure lanthanum compounds is best commenced with the liquid from which basic cerium sulphate has been for the first time precipitated by means of boiling acidified water. This liquid is boiled with natural pulverized magnesite, whereby the greater part of the dissolved cerium oxide is precipitated. After removing the precipitate, oxalic acid is added to the liquid acidified with hydrochloric acid; the precipitate which is formed is strongly heated in a porcelain crucible till the oxalic acid is decomposed; the oxides thus formed are dissolved in sulpphuric acid; the solution is evaporated, diluted with water, and again boiled with magnesite. The liquid, which yet contains traces of cerium, is

several times subjected to the same treatment with oxalic acid and magnesite; the oxides are at last dissolved in sulphuric acid, the acid is removed by evaporation, and the salt is heated to incipient redness. In order to obtain from this salt a lanthanum compound perfectly free from didymium, the original process of Mosander is the simplest and safest. The dehydrated sulphate is dissolved in small quantities of water at 0° to 5° C.; the liquid is warmed until the lanthanum salt precipitates in the form of a soft white mass of small needles; this mass is placed in a funnel heated to 100° C., and the mother liquor (which is set aside for the preparation of didymium salts) is removed by means of the water air-pump. The mass dehydrated at an incipient red heat is treated in the same way six or eight times. A layer 0·2 millim. thick of a concentrated solution of pure lanthanum sulphate shows no trace of the absorption-lines of didymium or erbium, nor, after conversion into chloride and testing in the spark-spectrum, any of the lines of cerium or yttrium. Samples of lanthanum oxalate, obtained from this material by fractional precipitation, exhibited, when converted into chlorides, no differences in the number, position, and relative intensity of their lines.

Lanthanum oxalate comports itself towards ammonium oxalate in the same manner as the corresponding cerium salt; the fractional precipitates from ammonium oxalate show the same spectra throughout. Lanthanum chloride gives no flame-spectrum; but it is characterized by a very brilliant spark-spectrum rich in lines, no. 14 *e*, Plate V.

The first mother liquor from the lanthanum sulphate is the starting-point in the preparation of pure didymium compounds. The methods hitherto put forward for the purifying of didymium compounds yield substances in which lanthanum is easily detected by the spark-spectrum. The removal of these impurities is carried out by me as follows:—If didymium sulphate, prepared according to Mosander's original directions, be decomposed by oxalic acid gradually added, the oxalates of didymium and lanthanum, which are at first precipitated, are again dissolved, until a point is reached when, upon the further addition of acid and shaking, the formation of a permanent crystalline amethyst-coloured precipitate commences. This first part of the precipitate is rich in didymium; it is separated from the other part, which contains larger quantities of lanthanum. The precipitate is converted into neutral dehydrated didymium sulphate, which is again treated in the way just described; and this treatment is repeated until the last product does not exhibit any lanthanum-lines in the spark-spectrum.

- The spark-spectrum of pure didymium chloride exhibits, as is

seen in Plate V. no. 15 *e*, faint lines in the neighbourhood of
70; but these lines are not sufficiently marked to serve as
a test for the presence of this substance.   The absorption-spec-
trum of the solid or dissolved didymium salts, however, is so
characteristic as to afford a means for detecting the smallest
trace of the element in any liquid which is not very strongly
coloured by foreign subtstances.   No. 15 *a*, Plate V., gives the
absorption-spectrum of a crystal of didymium sulphate 0·4
millim. in thickness.

Considerable doubt attaches itself to the purity of the
thorium oxide prepared according to the methods hitherto in
use.   An inquiry into the behaviour of perfectly pure thorium
chloride in the spark-spectrum-apparatus was therefore neces-
sary.

Orangite, from Brewig in Norway, was dissolved in hydro-
chloric acid; the silica was removed by evaporation, and the
metals precipitable by sulphuretted hydrogen by the passage of
that gas through the liquid; the solution was then oxidized by
nitric acid, precipitated by ammonia, and the precipitate di-
gested with oxalic-acid solution until the thorium oxalate pro-
duced became perfectly white.   The oxalate was washed, ignited,
and evaporated with concentrated sulphuric acid; the resulting
sulphate, after dehydration at a high temperature, was dissolved
in the smallest possible quantity of water at 0° to 6° C.   By
heating to 100° a precipitate was produced, which, when col-
lected on a filter, repeatedly washed with boiling water by
means of the water air-pump, and converted into chlorides,
showed the most prominent lanthanum-lines in the spark-spec-
trum.   All the portions of the sulphate obtained by fractional
precipitation at 100° C., as well as the mother liquor, showed
the lanthanum-lines; the last portion exhibited also the didy-
mium absorption-bands and the spark-lines of cerium.   Frac-
tional precipitation of the neutral sulphate by means of sodium
thiosulphate, yielded products which were not altogether free
from these impurities.   A new way had therefore to be devised
for the removal of these foreign substances.   The behaviour of
the oxalates of cerium, lanthanum, didymium, erbium, and
yttrium towards ammonium oxalate forms the basis of the
method.   A concentrated boiling solution of the latter salt
dissolves small quantities of the oxalates, which are entirely
reprecipitated on dilution with water and cooling; oxalate of
thorium, on the other hand, is easily dissolved under the same
conditions, and is not reprecipitated either on cooling, diluting,
or evaporating the liquid.   Thorium oxide remains when the
residue, obtained by evaporating this solution, is ignited in a
platinum dish.   By repeating this treatment with the sub-

stance, which has been already purified by several precipitations with sodium thiosulphate, pure thorium oxide may be obtained. When tested, as chloride, in the flame- and spark-spectrum, thorium affords no characteristic lines.

The discrepancies in the estimations of the atomic weight of thorium by different observers are probably to be accounted for by the presence of considerable impurities in the compound supposed to be thorium oxide.

For the preparation of pure compounds of yttrium and erbium I may refer to the method published by Bahr and myself [*]. The chlorides employed in the production of spectra were prepared from the material with which the atomic weight of yttrium was determined to be 30·85, and that of erbium to be 56·3. As proof of the complete separation of the two earths, it may be mentioned that the whole of the chlorides obtained from the fractional precipitates by oxalic acid yielded spectra in which the number and relative intensity of the individual lines were identical. The same thing was true with regard to the fractionally precipitated sulphates, and the last mother liquor remaining from these precipitations. In these observations I have looked in vain for traces of a third earth (terbia), which has been supposed by many chemists to exist in Gadolinite and to yield absorption-bands.

Yttrium and erbium chlorides give no flame-spectra; while the spark-spectrum of the former chloride is very rich in lines, Plate V. no. 12 *e*; that of the latter is shown in Plate V. no. 11 *e*. Erbium chloride, however, is much more easily recognized by the absorption-spectrum of its solution, Plate V. no. 11 *a*. Erbium oxide belongs to the small group of substances which yield a discontinuous spectrum when heated in the solid state; the clear lines no. 11 *f* correspond to the dark bands of the absorption-spectrum. This oxide may be thus easily and clearly distinguished from all other oxides.

Spectral analysis affords the easiest and most reliable means for recognizing the minerals which contain cerium and yttrium. The method to be adopted will be seen from the following examples.

1. *Cerite from the Bastnäs Quarry.*—A few centigrammes, evaporated with hydrochloric acid and taken up with the same acid and water, gave a concentrated solution which behaved as follows :—The light passing through it showed the characteristic absorption-spectrum of didymium, especially the bands 55 and 75 (no. 15 *a*, Plate V.). As no trace appeared of the characteristic absorption-bands of erbium, 35 (11 *a*), the absence of erbium in this cerite was proved. The carbon points saturated

* Liebig's *Ann.* vol. cxxxvii. p. 1.

with the liquid gave a spark-spectrum, in which 10 lanthanum
lines, and the cerium lines at 67·8, 70·9, 79·4 (no. 13 *e*),
appeared clearly; the spark-spectrum in hydrogen showed no
magnesium lines. The spectrum obtained by placing a platinum
wire, on which a little of the liquid had been allowed to eva-
porate in the non-luminous flame, exhibited faint calcium-lines
(no. 7 *a*); in the spark-spectrum the calcium line at 49 (7 *e*)
appeared very faintly.

2. *Gadolinite from Ytterby.*—The liquid obtained after sepa-
rating silica by evaporation with hydrochloric acid, gave the
didymium absorption-spectrum, the bands which appear at 55
(15 *a*) being especially marked : the absorption-bands of erbium
appeared faintly, especially 35 (11 *a*); 65 and 68 were very faint
but yet visible. The calcium-spectrum appeared slightly, and
the sodium-spectrum very faintly when the liquid was placed on
a platinum wire in the flame. The yttrium-lines appeared very
plainly in the spark-spectrum, the characteristic group of lines
between 40 and 50 (12 *e*) being especially visible. The cerium-
line 70·9 (13 *e*) appeared in the same spectrum ; but the lantha-
num lines did not appear. As the lanthanum-spectrum is
characterized by many and well-marked lines, it is evident that
the mineral under examination either contained no lanthanum
or but the smallest trace of that element. Yttrium, erbium,
didymium, cerium, calcium, and sodium were therefore found in
this mineral.

3. Very pure *orangite* from Brewig, treated as above described,
showed faintly the characteristic bands of didymium at 54 (15*a*).
The cerium-lines 70·9 and 67·5 (13 *a*), and the lanthanum-line
87 (14 *a*), were plainly visible in the spark-spectrum. The cal-
cium-spectrum appeared when the liquid was placed in the
flame.

4. *Wasite* yielded a solution in which the absorption-bands
of didymium, 55 and 75 (15 *a*), but no erbium bands appeared.
The flame-spectrum showed calcium-lines. The spark-spectrum
showed plainly only the cerium-line 70·9 (13 *e*). In conse-
quence of an excessive quantity of alumina and iron in the liquid,
the lines of the other earths only appeared after treating the
precipitate thrown down by ammonia with oxalic acid, igniting
the oxalates thus formed, and dissolving the residue in hydro-
chloric acid. A very intense yttrium-spectrum, along with the
characteristic cerium-lines 70·9 and 67·7 (13 *e*), was then ren-
dered visible. No trace of lanthanum was discoverable.

5. *Euxenite.*—A small quantity was fused with sodium car-
bonate, the mass was evaporated with hydrochloric acid and
taken up with the same acid diluted with water. The solution
was precipitated by ammonia, and the precipitate digested with

oxalic acid: a white heavy powder, consisting of the oxalates of the earths of the cerium and yttrium group, remained; this was treated with sulphuric acid, and the yttrium group separated from the cerium in the usual way. Traces only of the cerium group were obtained. The greater mass, consisting of the yttrium group, after conversion into chlorides, showed the absorption-bands of erbium very plainly, and also most distinctly the yttrium-lines in the spark-spectrum. The earths of the cerium group, also converted into chlorides, showed the didymium absorption-bands distinctly, but no recognizable traces of cerium or lanthanum in the spark-spectrum; a tolerably distinct yttrium spectrum showed the deficiency of the separation by the usual method with potassium sulphate.

LXII. *On some Identities derived from Elliptic-Function Formulæ.* By J. W. L. Glaisher, *M.A., F.R.S.**

I. I do not think any one has specially noticed a curious series of identities involving the expression

$$\text{arc} \tan q - \text{arc} \tan q^3 + \text{arc} \tan q^5 - \text{arc} \tan q^7 + \&c.,$$

which follow very simply from some of Jacobi's formulæ in the *Fundamenta Nova*.

Thus take the equation

$$\frac{1-q^2 \cdot 1-q^4 \cdot 1-q^6 \cdot 1-q^8 \cdots}{1-q \cdot 1-q^3 \cdot 1-q^5 \cdot 1-q^7 \cdots} = 1 + q + q^3 + q^6 + q^{10} + q^{15} + \&c.,$$

and write therein qi for q; also let $\alpha_1 = \text{arc} \tan q$, $\alpha_3 = \text{arc} \tan q^3$, &c.; then

$$\frac{1+q^2 \cdot 1-q^4 \cdot 1+q^6 \cdots}{\sqrt{(1+q^2)} \cdot \sqrt{(1+q^6)} \cdots (\cos \alpha_1 - i \sin \alpha_1)(\cos \alpha_3 + i \sin \alpha_3) \cdots}$$
$$= 1 - q^6 - q^{10} + q^{28} + q^{36} - \&c. + i(q - q^3 - q^{15} + q^{21} + q^{45} - \&c.),$$

and

$$\cos \{\text{arc} \tan q - \text{arc} \tan q^3 + \text{arc} \tan q^5 - \&c.\}$$
$$= \frac{1 - q^6 - q^{10} + q^{28} + q^{36} - \&c.}{\sqrt{(1+q^2)} \cdot \sqrt{(1+q^6)} \cdots 1 - q^4 \cdot 1 - q^8 \cdots},$$

$$\sin \{\text{arc} \tan q - \text{arc} \tan q^3 + \text{arc} \tan q^5 - \&c.\}$$
$$= \frac{q - q^3 - q^{15} + q^{21} + q^{45} - \&c.}{\sqrt{(1+q^2)} \cdot \sqrt{(1+q^6)} \cdots 1 - q^4 \cdot 1 - q^8 \cdots},$$

where the exponents in the numerators are the triangular num-

bers, and, after the first, the terms are alternately negative and positive in pairs.

Treating other formulæ in the *Fundamenta Nova* in the same manner, it will be found that we have

$$\text{arc tan } q - \text{arc tan } q^3 + \text{arc tan } q^5 - \text{arc tan } q^7 + \&c.$$

$$= \text{arc tan} \frac{q - q^3 - q^{15} + q^{21} + q^{45} - \&c.}{1 - q^6 - q^{10} + q^{28} + q^{36} - \&c.}$$

$$= \tfrac{1}{2} \text{arc tan} \frac{2q + 2q^9 + 2q^{25} + 2q^{49} + \&c.}{1 + 2q^4 + 2q^{16} + 2q^{36} + \&c.}$$

$$= \tfrac{1}{3} \text{arc tan} \frac{3q + 5q^3 - 11q^{15} - 13q^{21} + \&c.}{1 + 7q^6 - 9q^{10} - 15q^{28} + 17q^{36} + \&c.}$$

$$= \tfrac{1}{4} \text{arc tan} \frac{\dfrac{4q}{1-q^2} - \dfrac{4q^3}{1-q^6} + \dfrac{4q^5}{1-q^{10}} - \&c.}{1 - \dfrac{4q^2}{1+q^4} + \dfrac{4q^4}{1+q^8} - \dfrac{4q^6}{1+q^{12}} + \&c.}$$

In the third result the exponents are the triangular numbers, the signs are positive and negative in pairs, and the coefficients are the uneven numbers.

Denoting arc tan q — arc tan q^3 + arc tan q^5 — &c. by ψ, we can easily find also expressions for tan 6ψ, tan 8ψ, tan 12ψ, and tan 16ψ; for if P* denote what P becomes when qi is therein written for P, we have

$$\left(\frac{2\text{K}}{\pi}\right)^* = \left(\frac{1+q^2}{1-q^2} \cdot \frac{1-q^4}{1+q^4} \cdot \frac{1+q^6}{1-q^6} \dots\right)^2 (\cos 4\psi + i \sin 4\psi),$$

$$\left(\frac{2\sqrt{k}.\text{K}}{\pi}\right)^* = 2\sqrt[4]{i} \cdot \sqrt[4]{q} . 1 + q^2 . (1-q^4)^2 . 1 + q^6 \dots (\cos 2\psi + i \sin 2\psi);$$

and the formulæ for $\left(\dfrac{2\text{K}}{\pi}\right)^2$, $\left(\dfrac{2\text{K}}{\pi}\right)^3$, $\left(\dfrac{2\text{K}}{\pi}\right)$, $\left(\dfrac{2\sqrt{k}.\text{K}}{\pi}\right)^3$ are given in the *Fundamenta Nova*, pp. 103, 108, and 115; but the expressions are not of sufficient interest to make it worth while to write them down. I may, however, add the formula obtained for tan 4ψ by means of the value of $\left(\dfrac{2\sqrt{k}.\text{K}}{\pi}\right)^2$, for comparison with that given above, which is derived from the formula for $\dfrac{2\text{K}}{\pi}$. The numerator of tan 4ψ is found to be

$$\frac{q\sqrt{q}}{1+q^2} + \frac{3\sqrt{q^3}}{1+q^6} - \frac{5q^5\sqrt{q^5}}{1+q^{10}} - \frac{7\sqrt{q^7}}{1+q^{14}} + \&c.,$$

and the denominator

$$\frac{\sqrt{q}}{1+q^2} + \frac{3q^3\sqrt{q^3}}{1+q^6} - \frac{5\sqrt{q^5}}{1+q^{10}} - \frac{7q^7\sqrt{q^7}}{1+q^{14}} + \&c.,$$

the numerators of the fractions being alternately of the forms q^n and $q^n\sqrt{q^n}$.

There is another expression for $\tan\psi$ which is also worth noting. Putting qi for q in Euler's identity

$$1-q \cdot 1-q^2 \cdot 1-q^3 \cdot 1-q^4 \dots = 1-q-q^2+q^5+q^7-q^{12}- \&c.,$$

we find

$$\text{arc} \tan q - \text{arc} \tan q^3 + \text{arc} \tan q^5 - \&c.$$
$$= \text{arc} \tan \frac{q-q^5+q^7-q^{15}-\&c.}{1+q^2-q^{12}-q^{22}-q^{26}-\&c.},$$

the exponents being the uneven pentagonal numbers in the numerator, and the even pentagonal numbers in the denominator. In the numerator the sign of $q^N (N = \frac{1}{2}(3n^2 \pm n))$ is $(-)^{n+\frac{1}{2}(N-3)}$, and in the denominator $(-)^{n+\frac{1}{2}N}$.

II. In the formula

$$k' = \left(\frac{1-q \cdot 1-q^3 \cdot 1-q^5 \dots}{1+q \cdot 1+q^3 \cdot 1+q^5 \dots}\right)^4$$

put $\sqrt{q}$ for q, and

$$\frac{1-k}{1+k} = \left(\frac{1-\sqrt{q} \cdot 1-\sqrt{q^3} \cdot 1-\sqrt{q^5} \dots}{1+\sqrt{q} \cdot 1+\sqrt{q^3} \cdot 1+\sqrt{q^5} \dots}\right)^4;$$

change the sign of q, and

$$\frac{k'-ik}{k'+ik} = \left(\frac{1-i\sqrt{q} \cdot 1+i\sqrt{q^3} \cdot 1-i\sqrt{q^5} \dots}{1+i\sqrt{q} \cdot 1-i\sqrt{q^3} \cdot 1+i\sqrt{q^5} \dots}\right)^4,$$

whence

$$\text{arc} \tan \frac{k}{k'} = 4(\text{arc} \tan \sqrt{q} - \text{arc} \tan \sqrt{q^3} + \text{arc} \tan \sqrt{q^5} - \&c.),$$

which, since $\text{arc} \tan \dfrac{k}{k'} = \theta$, the angle of the modulus, is Jacobi's formula, "quæ inter formulas elegantissimas censeri debet" (*Fundamenta Nova*, p. 108). Replacing q by q^2, $\text{arc} \tan \dfrac{k}{k'}$ becomes

$$\text{arc} \tan \frac{1-k'}{2\sqrt{k'}} = \frac{\pi}{2} - 2 \text{ arc} \tan \sqrt{k'},$$

and we have

$\frac{1}{4}\pi -$ arc tan $\sqrt{k'} = 2$ (arc tan $q -$ arc tan $q^3 +$ arc tan $q^5 -$ &c.);
while if we put $\sqrt{q}$ for q, since

$$\text{arc tan } \frac{2\sqrt{k}}{1-k} = 2 \text{ arc tan } \sqrt{k},$$

we have

arc tan $\sqrt{k} = 2$ (arc tan $\sqrt[4]{q} -$ arc tan $\sqrt[4]{q^3} +$ arc tan $\sqrt[4]{q^5} -$ &c.),
both of which are deserving of notice.

The former of these two formulæ may also be written

am $\frac{1}{2}$ K $- \frac{1}{4}\pi = 2$ (arc tan $q -$ arc tan $q^3 +$ arc tan $q^5 -$ &c.).

LXIII. *The Application of Phosphorus to the "Poling" of Copper. By* WILLIAM WESTON, *Esq., Associate of the Royal School of Mines, and Chemist at H. M. Dockyard, Portsmouth.*

To William Francis, Esq., Ph.D.

Royal School of Mines,
MY DEAR FRANCIS,   Jermyn Street, Nov. 17, 1875.

I HAVE much pleasure in communicating to you for publication in the Philosophical Magazine the following account by Mr. William Weston, of the use of phosphorus in poling copper. The process, which originated with Mr. Weston, is novel, and, it seems to me, of much practical importance. A few months ago I visited Chatham, where it has been in operation for a year or two, in order to inquire concerning it, and the information which I obtained was highly satisfactory.

I remain

Very truly yours,
William Francis, Esq.   JOHN PERCY.

The use of phosphorus in that part of the operation of refining copper known as the poling process, which has been now for some time carried out in the Government metal-mills at Chatham Dockyard, is attended with such a marked effect on the metal as to render a short notice of it desirable, both on the ground of its scientific interest and probable practical importance.

The primary object of the addition of phosphorus at Chatham is to increase the density of the copper, with the expectation that the sheathing made from it for the navy would be thereby rendered more durable. Such increased density must greatly improve copper for many subsequent processes of manufacture; but, in

addition to a permanent effect on the metal itself, the phosphorus by its deoxidizing action actually poles it, thus accelerating the process and rendering unnecessary much use of the ordinary pole. When the experiment was first made the copper was at once overpoled, and had to be exposed again to the oxidizing action of the air. No attempt is made to effect the poling entirely by phosphorus; and this does not appear so desirable as to bring the metal near the tough-pitch point by its agency, and then complete the process by the less rapid action of the pole, as in this way the proper point at which the metal should be cast is more easily reached.

The phosphorus is added in the form of a rich compound of copper and phosphorus containing about 7 per cent. of phosphorus. This is prepared by pouring melted copper upon sticks of phosphorus coated with copper by immersion in a solution of cupreous sulphate*, and placed at the bottom of a conical-shaped iron crucible protected throughout by a lining of loam mixed with powdered coke. In the cover is a funnel through which the metal is poured, and a small hole for the escape of air. The metal falls on to a perforated plate coated like the crucible and placed midway between the bottom and cover, thus presenting a body of metal to absorb the escaping vapour of phosphorus.

The proportion of phosphorus required varies with the dryness of the copper after fusion. At Chatham 1 cwt. of the phosphide compound is added to a charge of 5 tons; and with this quantity the copper is scarcely ever overpoled. It represents about ·07 per cent. of phosphorus; and about half this quantity is found to be permanently retained.

The advantages of this use of phosphorus may be stated to be :—

1. Increased density of the copper, as will be seen from the following determinations :

Spec. grav.

Three samples of ordinary cake copper $\begin{cases} 8{\cdot}835 \\ 8{\cdot}835 \\ 8{\cdot}839 \end{cases}$

Copper compressed by Sir J. Whitworth 8·876

Copper after addition of phosphorus in refining-furnace . . . . . $\Big\}$ 8·854

If ·1 per cent. of phosphorus be added to copper melted in a crucible, its density is as great as that of compressed copper; but copper at the proper pitch will only roll hot, and without overpoling it, no larger addition than that stated above can be made in the refining-furnace.

2. Acceleration of the process of poling.

3. Reduced consumption of poles.

* At Chatham uncoated sticks of phosphorus are now used.

LXIV. *On the Mathematical Principles of* Laplace's *Theory of Tides. By* Professor CHALLIS, *M.A., F.R.S., F.R.A.S.*[*]

IN three articles, contained in the Numbers of the Philosophical Magazine for January, April, and June, 1870, I have discussed the problem of the tides of an unbounded ocean of small uniform depth, including in the discussion several tentative solutions of the problem, one of which I finally considered to be essentially correct. In the course of endeavours to make the logic of that solution as exact as possible, I arrived at the proof of a general proposition relating to oceanic tides, which, at the same time that it answers the purpose intended, applies also in an important manner to Laplace's mathematical theory of tides. The proposition may be thus enunciated:—Whatever be the circumstances of the tides, it is *necessary* to suppose that $u dx + v dy + w dz$ is an exact differential, the velocities u, v, w being resolved parts, parallel to rectangular axes fixed relatively to the earth, of the tidal motion at the point whose coordinates are x, y, z. The proof above referred to is that which I now proceed to give.

Since, on the principle of the supposition of small periodic motions, the tides of the ocean, whatever be the forms of its bottom and boundaries, are the sum of the tides which the sun and moon would produce separately, we may calculate the effect of the attraction of each luminary by itself. Supposing at first the attracting body to be the moon, let the earth's centre, conceived to be fixed in space, be the origin of rectangular coordinates, its axis coincide with the axis of z, and the plane of its equator coincide with that of xy; and let the axes of x and y have given directions in space. Then if x_1, y_1, z' be the coordinates of the moon's centre referred to these axes at any time t reckoned from a given epoch, the values of these coordinates are obtainable from the Lunar Theory as functions of t. Let the axes of x and y be now changed to others in the same plane, but *fixed with reference to the rotating earth*, and let the positive direction of z be from the plane of the equator *northwards*, that of y from the earth's centre to *the meridian of Greenwich*, and that of x from the plane of that meridian *eastward*. Then, supposing the new coordinates to be x' and y', it will be found (the earth's rotation being eastward) that

$$x' = x_1 \cos(\omega t + \alpha) - y_1 \sin(\omega t + \alpha),$$
$$y' = x_1 \sin(\omega t + \alpha) + y_1 \cos(\omega t + \alpha),$$

ω being the rate of rotation, and α an arbitrary constant such that if t_1 be the epoch of coincidence of the two sets of coordi-

nates, $\omega t_1 + \alpha = 0$. Hence x' and y' are functions of t that may be calculated.

It has thus been shown that the coordinates x', y', z' are all expressible as functions of t. We may now, if we please, suppose the earth to have no motion of rotation, provided an angular motion about the axis of z equal to that of the earth be impressed on the moon at each instant in the direction from east to west; for thus the relative motions, and the values of x', y', z' will remain the same as before, only these coordinates will *apparently* be referred to axes fixed relatively to the earth and fixed in space, as plainly may be the case considering that the earth is apparently absolutely motionless. Let x, y, z be the coordinates of any point of the ocean referred to the same axes. Then, if m be the attraction of the moon at the unit of distance, and R be put for

$$\frac{m(xx' + yy' + zz')}{(x'^2 + y'^2 + z'^2)^{\frac{3}{2}}} - \frac{m}{\left((x-x')^2 + (y-y')^2 + (z-z')^2\right)^{\frac{1}{2}}},$$

we shall have, as in Physical Astronomy, for the attractions in the directions of the axes of x, y, z, the values $\dfrac{d\mathrm{R}}{dx}$, $\dfrac{d\mathrm{R}}{dy}$, $\dfrac{d\mathrm{R}}{dz}$ respectively.

We have next to take account of the attraction of the earth itself at the same point of the ocean. The amount and direction of this force will depend on the composition and dimensions of the earth, and on the form of its superficies as determined by the mutual attractions of its parts, both solid and fluid, by the moon's attraction, by centrifugal force, and by its superficial irregularities. Now, as we are here concerned only with *central* forces emanating according to the law of gravity at each instant from all the elements of the earth's mass, if we call the resultant forces in the directions of the axes of coordinates X_1, Y_1, Z_1, we shall have, as is known, $X_1 dx + Y_1 dy + Z_1 dz$, a complete differential. The values of X_1, Y_1, Z_1 may contain t.

It only remains to take into consideration the action of *centrifugal force*. It is here to be observed that we have already in the previous reasoning had regard to the *hydrostatical* effect of this force in modifying the earth's figure, and thereby exerting an indirect influence on the amount of tide by altering the earth's attraction at a given point of the ocean. At present we have to take account of the *hydrodynamical* effect of centrifugal force— that is, the effect of its action on *fluid in motion*, the motion being such as is peculiar to a fluid, causing the forms and relative positions of the elementary parts to be continually changing. Tidal motion is plainly of this kind. The centrifugal force in

the direction of x is $\omega^2 x$; that in the direction of y is $\omega^2 y$; and parallel to the axis of z it is zero.

Now let X, Y, Z be respectively the sums of the impressed forces parallel to the axes of coordinates. Then, from what has been shown above,

$$X = X_1 + \omega^2 x + \frac{d\mathrm{R}}{dx},$$

$$Y = Y_1 + \omega^2 y + \frac{d\mathrm{R}}{dy},$$

$$Z = Z_1 \qquad + \frac{d\mathrm{R}}{dz};$$

whence it follows that $X dx + Y dy + Z dz$ is an exact differential, which for brevity I shall call $(d\mathrm{F})$.

This being understood, we have, in the next place, by the third general equation of hydrodynamics,

$$(dp) = \left(X - \left(\frac{du}{dt}\right)\right)dx + \left(Y - \left(\frac{dv}{dt}\right)\right)dy + \left(Z - \left(\frac{dw}{dt}\right)\right)dz,$$

in which u, v, w have the significations defined in the first paragraph of this communication, the motions under consideration being relative to the earth supposed fixed. From this equation it is readily seen that

$$\left(\frac{du}{dt}\right)dx + \left(\frac{dv}{dt}\right)dy + \left(\frac{dw}{dt}\right)dz = (d\mathrm{F}) - (dp).$$

Hence if, as is usually done, the investigation is restricted to the first powers of u, v, and w, it will follow, since the right-hand side of this equality is an exact differential, that

$$\frac{du}{dt}\,dx + \frac{dv}{dt}\,dy + \frac{dw}{dt}\,dz$$

is also an exact differential. To satisfy this condition it is *necessary* and sufficient to assume that $u dx + v dy + w dz$ is an exact differential; for in that case, supposing $(d\phi)$ to be the differential,

$$\frac{du}{dt} = \frac{d^2\phi}{dx\,dt}, \quad \frac{dv}{dt} = \frac{d^2\phi}{dy\,dt}, \quad \frac{dw}{dt} = \frac{d^2\phi}{dz\,dt},$$

and

$$\frac{du}{dt}\,dx + \frac{dv}{dt}\,dy + \frac{dw}{dt}\,dz = \left(d\,.\,\frac{d\phi}{dt}\right).$$

Similarly, if u', v', w' be the resolved parts of the tidal motion at the same position due to the sun's attraction, we should have $u' dx + v' dy + w' dz$ an exact differential. If, therefore, U, V, W be the resolved velocities due to the combined action of sun and

moon, it will follow, since $U = u + u'$, $V = v + v'$, $W = w + w'$, that

$$U dx + V dy + W dz$$

is an exact differential.

The general conclusion from this argument is that, for the calculation of tidal motion *in any case whatever*, $u dx + v dy + w dz$ must be assumed to be an exact differential, the calculation being restricted to terms of the first order, and u, v, w being the components, parallel to axes fixed relative to the earth, of the tidal motion at xyz, as it might at any time be actually observed and measured.

In the *Mécanique Céleste*, liv. i. chap. viii. art. 34, Laplace asserts that " in the theory of the flux and reflux of the sea we cannot suppose $u dx + v dy + w dz$ to be an exact differential, because it is not such in the very simple case in which the sea has no other movement than that of rotation which is common to it with the earth." It is true that if the values of u, v, w at any point include resolved parts of the velocity at that point due to the motion of rotation which the ocean partakes of in common with the earth, the expression $u dx + v dy + w dz$ cannot be an exact differential. But this circumstance affects in no manner the foregoing argument, by which I have shown, *after* fully taking account of centrifugal force, and abstracting from the motion of rotation, that if u, v, w be resolved parts exclusively of tidal motion, $u dx + v dy + w dz$ will be an exact differential. Such values of u, v, w are in fact just those which a theory of tides is required to find.

Consequently, if $(d\phi)$ be put for $u dx + v dy + w dz$, so that the equation of constancy of mass becomes

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0, \quad \ldots \quad \ldots \quad (\alpha)$$

this equation is perfectly general, as respects the problem of tides. By making use of it, a solution of that problem much simpler than that proposed by Laplace would probably be obtained. I cannot forbear expressing the opinion that the difference between the views recently expressed in this Journal by two eminent mathematicians relative to certain points of Laplace's theory, may be traceable to the needless complexity of the mathematics of that theory, and might be expected to disappear if the simplification indicated above were adopted.

In vol. xxxix. of the Philosophical Magazine (April 1870, p. 260) I have given a solution of the problem of the tides of an unbounded ocean of uniform small depth on the supposition that the moon revolves about the earth in the plane of the equator at the mean distance with the mean angular motion, and I have

adduced reasons, which are not altogether satisfactory, for employing the equation (α) for that purpose. The use of that equation I consider to be completely justified by the argument contained in the present communication.

Again, supposing λ to be the north latitude and θ the longitude west from Greenwich of any particle of the ocean distant by *r* from the earth's centre, the above-mentioned solution is effected by assuming that

$$\phi = \mathrm{F}(r)\cos^2\lambda \sin 2(\theta - \mu t).$$

Having been led to adopt this form of φ by indirect considerations, for verification I substituted it in the equation (α), and thus obtained a differential equation containing only F(*r*) and *r*, the integration of which gave $\mathrm{F}(r) = \mathrm{C}r^2 + \dfrac{\mathrm{C}'}{r^3}$. Thus the truth of the above expression for φ was proved by an argument *à posteriori*. The proper course for arriving at the same expression *directly* would be to obtain a solution of (α) suitable to the given circumstances by means of Laplace's *coefficients*.

To these remarks relative to the above-mentioned problem I have only to add that I consider the mathematical details of the solution contained in pp. 261–266 of vol. xxxix. of the Phil. Mag. to be quite correct, excepting that the expressions for X and Y in p. 262 should have included the terms $\omega^2 x$ and $\omega^2 y$ respectively, on account of centrifugal force.

Cambridge, November 20, 1875.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 152.]

February 25, 1875.—Joseph Dalton Hooker, C.B., President, in the Chair.

THE following communication was read:—

   "On the Forms of Equipotential Curves and Surfaces and Lines of Electric Force."—The Bakerian Lecture. By Prof. W. G. Adams, M.A., F.R.S.

This paper contains an account of certain experimental verifications of the laws of electrical distribution in space and in a plane conducting sheet.

The potential at any point of an unlimited plane sheet due to a charge of electricity at any other point of the plane at distance *r* from it is proportional to the logarithm of the distance; and the potential due to two or more charges at different points of the plane is the sum of the potentials due to the several charges; so that when there are two points in a plane conducting sheet con-

nected with the poles of a battery, as there are equal currents flowing at those two points, one into and the other out of the sheet, the potential at any point of the sheet is proportional to the difference of the logarithms of its distances from the two points or electrodes where the current enters and leaves the sheet.

The potential is constant for a series of points if the difference of the logarithms of the distances of each of those points from the electrodes remains constant, *i. e.* if the ratio of the distances of each of those points from the electrodes remains constant.

The curve joining this series of points is an equipotential curve.

If r and r_1 are the distances of any point in the curve from the two electrodes, and c a constant, then

$$r = cr_1.$$

Hence the equipotential curves are circles with their centres on the line joining the two electrodes; and the lines of force which cut the equipotential curves at right angles are also arcs of circles passing through the two electrodes.

The lines of force may be regarded as distinct from one another, but as filling up all the space on the conductor between the two electrodes; and the distribution would not be altered if we conceive of them as divided from one another like separate wires conducting currents side by side. By taking out any space bounded by lines of force, we shall increase the quantity flowing along the other lines of force, but shall not alter the distribution of the current among them. Hence we may cut out a disk from an unlimited sheet without altering the form of the lines of force, if the boundary of the disk be arcs of circles passing through the two electrodes; so that for a circular disk with the electrodes on the edge of it, the equipotential curves are circles having their centres on the straight line joining the electrodes.

The forms of the equipotential curves may be traced out experimentally by attaching two battery-electrodes to a disk of tinfoil, and having two similar electrodes attached to a delicate galvanometer; one of these electrodes being fixed at a point through which the equipotential curve is to be drawn, the other may be moved from point to point to trace out the successive points, so that no current may pass through the galvanometer. A comparison of the experimental results with the theory shows a complete agreement.

In a large square sheet 310 millims. in diameter, with the electrodes 126 millims. apart, the curves in the centre and near the electrodes, which are drawn by pricking fine holes through the tinfoil on a sheet of paper below, are very accurately circular, and mostly coincide with circles, until the points are so far from the centre that the form of the equipotential curves is affected by the edge of the disk. In a circular disk with the electrodes on the edge subtending 60° at the centre, the experimental curves are shown to be accurately arcs of circles, with their centres on the line joining the electrodes.

In an unlimited sheet, when there are several electrodes by

which currents enter and leave the sheet, the potential at any point is

$$A \log \left(\frac{r \; r' \; r'' \; \cdots}{r_1 r_1' r_1'' \; \cdots} \right),$$

where $r, r', r'' \ldots$ are the distances to the electrodes of one kind, and r_1, r_1', r_1'' are the distances to the electrodes of the other kind. Taking the case of one positive electrode at the centre and four negative electrodes round it at the corners of a square, the curves are traced and are seen to be the same as the curves at the corner of a square sheet with a positive electrode at the corner and two negative electrodes on the edges; the curves are also the same for a square sheet with a positive electrode at the corner, and one negative electrode along the diagonal.

The equation for these equipotential curves is

$$r^4 = c r_1 r_2 r_3 r_4,$$

and is derived, in the case of the limited sheets, by considering that, to every electrode on the limited sheet, there corresponds an equal and like electrode at each of the electrical images of that electrode formed by the edges of the sheet. If we trace the curves for this arrangement of electrodes in the unlimited sheet, the edges of the limited sheet will be some of the lines of force; and so we may divide the sheet along these edges, without altering the form of the equipotential curves. Where an electrode and its images coincide in position, the index of r is equal to one more than the number of images.

When there are four electrodes, two of each kind on an unlimited sheet, an equipotential curve is given by the equation

$$r r' = c r_1 r_1'.$$

If the four points lie on a circle, and the complete quadrilateral be drawn through them, the circles which have their centres at the intersections of opposite sides of the quadrilateral, and which cut the first circle at right angles, will also cut one another at right angles. One of these circles is shown to be an equipotential curve for the four electrodes, and the other is a line of force.

Hence, if we cut the unlimited sheet along the edge of this latter circle, we shall not alter the forms of the equipotential curves; and within it we shall have one electrode of each kind, the others being their electric images, the product of the distances of an electrode and its image from the centre being equal to the square of the radius of the disk. If an electrode is at the edge of the disk, then the electrode and its image coincide, and the equation to the equipotential curve is

$$r^2 = c r_1 r_2.$$

When one pole is at the edge and the other is at the centre of a circular disk, since the electric image of the centre is at an infinite distance, the equation to the equipotential curves is

$$r^2 = c r_1.$$

This is an interesting case, as showing that the equipotential curves do not always cut the edge of the disk at right angles. The curves around the centre of the disk are nearly ellipses of small eccentricity, with one focus in the centre; but on placing one tracing electrode at a distance from the centre

$$= (3 - 2\sqrt{2})a,$$

between the electrodes, where a is the radius, there is great uncertainty in determining the form of the curve on the opposite side of the centre of the disk.

This is explained by the fact that the electrodes were 1 millim. in diameter, and a difference of distance of 1 millim. between the electrodes near this point corresponds to a large portion of the disk on the other side of the centre—this portion including an area of about 500 square millims. in a circle 36 millims. in radius, *i. e.* about one eighth of the whole area of the circle. On placing one of the galvanometer-electrodes at the extremity of the diameter through the battery-electrodes and tracing with the other, it is found that the equipotential curve through that point cuts the edge of the disk at an angle of 45°, and that there are two branches cutting one another at right angles.

These peculiarities are explained on tracing the curve

$$r^2 = 4ar_1$$

corresponding to this case. The extremity of the diameter is a point through which two branches of the curve pass at right angles to one another.

The forms of the equipotential surfaces and lines of force in space may be determined experimentally by taking a large vessel containing a conducting liquid and placing two points, the ends of two covered wires, for battery-electrodes, at a given depth in the liquid and away from the sides and ends of the vessel, taking similar covered wires, immersed to the same depth, for galvanometer-electrodes.

For two electrodes, the equipotential surfaces will be surfaces of revolution around the straight line joining them, and so will cut any plane, drawn through this straight line or axis, everywhere at right angles.

Hence we may suppose sections of the liquid made along such planes without altering the forms of the equipotential surfaces. This shows that we may place our battery-electrodes at the side of a rectangular box containing the liquid, and with the points only just immersed below the surface of the liquid; and the equipotential surfaces will be the same as if the liquid were of unlimited extent in every direction about the electrodes.

We shall obtain the section of the equipotential surface by taking for galvanometer-electrodes two points in the surface of the liquid, keeping one fixed and tracing out points of equal potential with the other.

The potential at any point in space, due to two equal and oppo-

site electrodes, is

$$A\left(\frac{1}{r} - \frac{1}{r_1}\right),$$

where r and r_1 are the distances of the point from the electrodes; so that for an equipotential surface

$$\frac{1}{r} - \frac{1}{r_1} = \text{constant.}$$

These surfaces are cut at right angles by the curves $\cos\theta - \cos\phi = c$, which are also the magnetic lines of force, θ and ϕ being the angles which the distances from the electrodes make with the axis. That the lines of force in a vessel of finite size should agree with the lines of force in space, the form of the boundary of the vessel in a plane through the axis should everywhere be a line of force; but the ends of a rectangular vessel coincide very closely with certain lines of force, either when the electrodes are at the ends, or when there are two electrodes within the vessel, and two supposed electrodes at their electrical images at an equal distance outside the ends of the vessel.

The equipotential surfaces are given in this case by the equation

$$\frac{1}{r} + \frac{1}{r'} - \frac{1}{r_1} - \frac{1}{r_1'} = \text{constant,}$$

and the lines of force by the equation

$$\cos\theta + \cos\theta_1 - \cos\phi - \cos\phi_1 = c.$$

The curve for which $c = 2$ coincides very closely with the ends of the box.

The equipotential surfaces were traced out in sulphate of copper and in sulphate of zinc by the following method :—

A rectangular box was taken, and the battery-electrodes attached to pieces of wood, which could be clamped at the centre of the end of the box, and could be brought to any required point in the line joining the middle points of the end of the box. The galvanometer-electrodes were attached to $\top$ pieces which rest on the ends and side of the box, and the position of the electrodes was read off by a millimetre-scale placed on the ends and sides of the box.

In the sulphate-of-copper experiments, covered wire with the end exposed was immersed to half the depth of the liquid; in the experiments with sulphate of zinc, the zinc electrodes were just immersed below the surface of the liquid. The close coincidence between the experimental curves traced out and the theoretical curves and surfaces in space is shown by a comparison of the numbers given in the paper for several of the curves which have been traced out; it also shows that, by reversing currents alternately, it is easy to keep the polarization very small, and of constant amount, on the galvanometer-electrodes.

When the electrodes are parallel lines extending throughout the depth of the liquid, the equipotential surfaces are cylindrical, and their sections are given by the equation

$$\log(r\,r'\ldots) - (\log r_1\,r_1'\ldots) = \log c,$$

where there are several positive and several negative electrodes, $r \cdot r' \ldots$ &c. being measured from the points where the electrodes cut the plane of the section.

Hence the forms of these equipotential curves are the same as in a plane sheet; so that the forms traced out in tinfoil will be the same as the corresponding forms in space for line electrodes. These forms may be traced out in sulphate of copper with copper electrodes, or in sulphate of zinc with amalgamated zinc electrodes; and for these experiments, with cylindrical and other vessels, the polar coordinates may be measured directly. One of the battery-electrodes is made the origin of coordinates, and a lath, or brass wire, resting on the edges of the vessel has a slot along it, the origin being at one end or at some point of the slot. In the slot is a sliding piece of wood or ivory which carries one of the galvanometer-electrodes, and the lath is capable of turning about the battery-electrode on which it is placed. Around this electrode is a graduated circle for measuring the angles about the origin; and on the sides of the slot is a millimetre-scale for measuring the distances from the origin.

The other galvanometer-electrode may be fixed in a manner which is most suitable in each case.

The results of these investigations show how closely the experimental determination of equipotential surfaces and lines of force agrees with the theory of electrical distribution in space.

March 18.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—

"On the Liquation, Fusibility, and Density of certain Alloys of Silver and Copper." By W. Chandler Roberts, Chemist of the Mint.

The author states that the most remarkable physical property of silver-copper alloys is a molecular mobility, in virtue of which certain combinations of the constituents of a molten alloy become segregated from the mass, the homogeneous character of which is thereby destroyed. These irregularities of composition have long been known, and reference is made to them in the works of Lazarus Erckern (1650) and of Jars (1774). A very complete memoir was published in 1852 by Levol, who did much towards ascertaining the nature and defining the limits of this molecular mobility. He discovered the important fact that an alloy containing 71·89 per cent. of silver is uniform in composition. Its chemical formula $(Ag_3 Cu_2)$ and peculiar structure led him to conclude that all other alloys are mixtures of this, with excess of either metal.

The electric conductivity of these alloys was studied in 1860 by Matthiessen, who doubted the accuracy of Levol's theory, and viewed them as "mechanical mixtures of allotropic modifications of the two metals in each other."

The author then described the experiments he made with a

view to determine the melting-points of a series of these alloys. He adopted Deville's determination of the boiling-point of zinc (1040° C.) as the basis of the inquiry, and ascertained, by the method of mixtures, the mean specific heat of a mass of wrought iron between 0° C. and the melting-point of silver, which, as Becquerel showed, is the same as the boiling-point of zinc.

The mean of three experiments, which were closely in accordance, gave 0·15693 as the specific heat; and it should be pointed out that this number includes and neutralizes several errors which would affect the accuracy of the subsequent determinations.

The melting-points of several alloys were then determined by plunging an iron cylinder into them and transferring the iron to a calorimeter. These melting-points varied from 840° C. to 1330° C., or through a range of 490° C. The alloys which occupy the lowest portion of the curve contain from 60 to 70 per cent. of silver. The results are interesting, as they show that the curves of fusibility and electric conductivity are very similar.

The author states that, in studying the phenomena of liquation, the alloys were cast in red-hot moulds of firebrick in which the metal (about 50 oz.) could be slowly and uniformly cooled. The results showed that the homogeneity of Levol's alloy is slightly disturbed by this method of casting; and, on the other hand, that alloys which contain more than 71·89 per cent. of silver hardly show signs of rearrangement when the solidification is gradually effected. Two alloys were examined, which contained 63 and 33·3 per cent. of silver respectively. Both were found to be far from homogeneous. In the case of the former the arrangement was influenced by gravity, the base of the casting being rich in silver.

The density of pure silver and of Levol's homogeneous alloy while in the fluid state were then determined by the method described by Mr. Robert Mallet[*], the metals being cast in conical vessels of wrought iron. The results obtained were as follows:—

	Density fluid.	Density solid.
Pure silver	9·4612	10·57
Levol's alloy	9·0554	9·9045

In the case of silver, the mean linear expansion deduced from this change of density is ·00003721 per 1° C., which is nearly double the coefficient at temperatures below 100° C.

April 15.—Joseph Dalton Hooker, C.B., President, in the Chair.

The following communication was read:—

"Researches upon the Specific Volumes of Liquids." By T. E. Thorpe.

I. *On the Atomic Value of Phosphorus.*

Hermann Kopp has shown that, as a rule, the specific volume of

* Proc. Roy. Soc. vol. xxiii. p. 209.

an element is invariable when in combination. Exceptions to the law occur, however, in the cases of oxygen and sulphur, each of which bodies has two specific volumes dependent upon the manner in which they are held in union. When contained "within the radicle," as in acetyl, C_2H_3O, oxygen has the value 12·2, but when existing "without the radicle," as in alcohol, it has the smaller value, 7·8. Sulphur, when "within the radicle," has the specific volume 28·6 ; when "without the radicle," it has the specific volume 22·6.

The cause of these variations may be thus stated in the language of modern theory :—When dyad sulphur and oxygen are united to an element by both their affinities, their specific volumes become respectively 28·6 and 12·2 ; when they are attached by only one combining unit, their specific volumes are 22·6 and 7·8.

Phosphorus is regarded by certain chemists as invariably a triad ; others maintain that it is sometimes a triad, at other times a pentad. In the trichloride it is a triad, in the oxychloride and thiochloride it is a pentad. According to this view, the two latter compounds possess the following constitution :—

$$\begin{array}{cc} \mathrm{Cl} & \mathrm{Cl} \\ | & | \\ \mathrm{Cl-P=O} & \mathrm{Cl-P=S} \\ | & | \\ \mathrm{Cl} & \mathrm{Cl} \end{array}$$

If, however, phosphorus is invariably trivalent, the oxychloride and thiochloride must possess the formulæ

$$\begin{array}{cc} \mathrm{Cl} & \mathrm{Cl} \\ | & | \\ \mathrm{P-O-Cl} & \mathrm{P-S-Cl} \\ | & | \\ \mathrm{Cl} & \mathrm{Cl} \end{array}$$

It is possible to decide between the two modes of representing the constitution of these compounds, if it be granted that the variation in the specific volume of oxygen and sulphur is due to the manner in which these elements are held in union. For, if the phosphorus in the oxychloride and thiochloride be quinquivalent, the oxygen and sulphur must possess the greater of the two values, since both their combining units are united to the phosphorus ; if, on the other hand, phosphorus be trivalent, the oxygen and sulphur must possess the smaller of the two values.

The author has determined the specific gravity, boiling-point, and rate of expansion of PCl_3, $POCl_3$, and $PSCl_3$, in order to ascertain the specific volume of the oxygen and sulphur in the two latter compounds, and consequently the chemical value of the phosphorus ; and he finds that the specific volumes of the oxygen and sulphur are almost identical with the values given by Kopp for these elements when "without the radicle." It would therefore appear that the oxychloride and thiochloride must possess the

constitution

$$\begin{array}{ccc} \text{Cl} & & \text{Cl} \\ | & & | \\ \text{P} - \text{O} - \text{Cl} & & \text{P} - \text{S} - \text{Cl} \\ | & & | \\ \text{Cl} & & \text{Cl} \end{array}$$
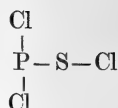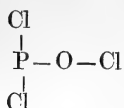
and that the phosphorus in these bodies is to be regarded as a triad.

The author concludes by discussing Buff's hypothesis, that the specific volume of an element varies with its chemical value; and he shows that, in the case of phosphorus, there are no reasons for the belief that this element has a variable specific volume.

GEOLOGICAL SOCIETY.

January 27, 1875.—John Evans, Esq., F.R.S., President, in the Chair.

The following communications were read :—

1. " On the Structure and Age of Arthur's Seat, Edinburgh." By John W. Judd, Esq., F.G.S.

The author said that Arthur's Seat, so long the battle-ground of rival theorists, furnished in the hands of Charles Maclaren a beautiful illustration of the identity between the agencies at work during past geological periods and those in operation at the present day.

One portion, however, of Maclaren's masterly exposition of the structure of Arthur's Seat, that which requires a *second* period of eruption upon the same site, but subsequent to the deposition, the upheaval and the denudation of the whole of the Carboniferous rocks, is beset with the gravest difficulties. The *Tertiary* and *Secondary* epochs have in turn been proposed and abandoned as the period of this supposed second period of eruption ; and it has more recently been placed, on very questionable grounds, in the *Permian.*

The antecedent improbabilities of this hypothesis of a second period of eruption are so great, that it was abandoned by its author himself before his death. A careful study of the whole question by the aid of the light thrown upon it in comparing the structure of Arthur's Seat with that of many other volcanoes, new and old, shows the hypothesis to be alike untenable and unnecessary.

The supposed proofs of a second period of eruption, drawn from the position of the central lava column, the nature and relations of the fragmentary materials in the upper and lower parts of the hill respectively, and the position of certain rocks in the Lion's Haunch, all break down on reexamination ; while, on the other hand, an examination of Arthur's Seat, in connexion with the contemporaneous volcanic rocks of Forfar, Fife, and the Lothians, shows that in the former we have the relics of a volcano which was at first submarine but gradually rose above the Carboniferous sea, and was the product of *a single* and almost continuous series of eruptions.

2. " The Glaciation of the Southern part of the Lake-district, and the Glacial origin of the Lake-basins of Cumberland and Westmoreland."—Second Paper. By J. Clifton Ward, Esq., F.G.S.

The directions of ice-scratches in the various dales having been pointed out, the course of the several main glaciers were described ; and it was shown how they must have become confluent in all the lower ground, forming a more or less continuous ice-sheet, which overlapped most of the minor ridges parting valley from valley, and was frequently forced diagonally across them.

The positions of certain ice-grooves having an abnormal direction were described ; in several cases these cross lofty ridges at right angles to their direction, and generally at passes or depressions along a watershedding line. Most of those noticed had a generally east-and-west direction, and occurred at varying heights, from 1250 ft. to 2400 ft. The author, while acknowledging the difficulty attendant upon *any* explanation, was inclined, though somewhat doubtfully, to regard these abnormal markings as due to floating ice during the great period of interglacial submergence.

The moraines were all believed to belong to the last set of glaciers.

The subject of the " Glacial origin of Lake-basins " was then entered upon, and the following lakes discussed by means of diagrams drawn to scale, showing lake-depths, mountain-outlines, and the thickness of the ice—Wastwater, Grasmere, Easdale, Windermere, Coniston, and Esthwaite, together with several mountain-tarns. In the case of Wastwater, the bottom was shown to run below the level of the sea for a distance of a mile and a quarter, and the deepest point to be just opposite the spot at which the only side valley joins the main one. While the greatest depth of the lake is 251 ft., the thickness of the old glacier-ice must have been fully 1500 ; and, all points considered, Prof. Ramsay's theory of glacial erosion seemed to the author certainly to be upheld. In like manner the same theory was thought to account for the origin of the other lakes mentioned, such ones as Windermere and Coniston being but long narrow grooves formed at the bottom of preexisting valleys.

Mountain-tarns were held to be due sometimes wholly to glacial erosion, sometimes to this combined with a moraine dam, and occasionally to the ponding back of water by moraines alone, or moraine-like mounds formed at the foot of snow-slopes.

February 10, 1875.—John Evans, Esq., F.R.S., President, in the Chair.

The following communication was read :—

2. " On the Bone-caves in the neighbourhood of Castleton, Derbyshire." By Rooke Pennington, Esq., LL.B. Communicated by Prof. W. Boyd Dawkins, F.R.S., F.G.S.

The author described as a Prehistoric Cave, the Cave-dale Cave, situated in Cave-dale, just below the keep of Peveril Castle. The

upper earth in this cave contained fragments of late pottery mixed up (by rabbits) with bits of rude prehistoric pottery, a tooled piece of stag's horn, an iron spike, two worked flints, a piece of jet, part of a bone comb, and a bronze celt of peculiar form, many bones of *Bos longifrons* and goat, broken to get out the marrow, and remains of hogs; charcoal and human teeth also attested the occupation of the cave by man. There were also remains of fox, badger, cat, water-rat, dog, red deer, duck, fowl, and hare. Lower down were remains of *Bos longifrons,* hog, red deer, wolf, and horse; and lower still, next the rock, more human teeth, remains of animals, and a good flint. The cave seemed to have been occupied from time to time during a lengthened period, probably from the Neolithic age into those of bronze and iron. A cave in Gelly or Hartle Dale, contained, in blackish mould, bones (some broken) of goat, pig, fox, and rabbit, and pieces of very rude prehistoric pottery.

Of Pleistocene caves and fissures the author described several. One in Hartle Dale furnished remains of rhinoceros, aurochs (*Bison priscus*), and mammoth, lying in yellow earth. The bones were probably carried in by water. A fissure near the village of Waterhouses, in Staffordshire, is 6 feet wide, and filled with the ordinary loam. Bones of mammoths and the skeleton of a young bison have been obtained from it; and the author supposes the animals to have fallen into the fissure while making for the river to drink. The Windy-Knoll fissure is situated near Castleton, in a quarry near the top of the Winnetts, and close to the most northern boundary of the mountain-limestone of Derbyshire. The author described particularly the situation of this fissure and drainage of the district in which it is situated. The fissure itself is filled with the ordinary loam, containing fragments of limestone, and enclosing an astonishing quantity of bones of animals confusedly mixed together, those lowest down near the rocks being coated with and sometimes united by stalagmite. The author supposes that this was a swampy place into which animals fell from time to time, and in rainy seasons their remains might be washed into it from the neighbouring slopes.

LXVI. *Intelligence and Miscellaneous Articles.*

ON THE DENSITIES OF PURE PLATINUM AND IRIDIUM AND THEIR ALLOYS. BY H. SAINTE-CLAIRE DEVILLE AND H. DEBRAY.

THE precise determination of the density of bodies is of high importance for science, because the various isomeric and allotropic states of one and the same substance are each manifested by a particular value of the density. In the present case it is especially so, on account of the diverse properties possessed by metals from the platinum-ores according to their physical state and their mode of preparation.

1. *Platinum.*—It is very difficult, by known processes, to remove from impure platinum the iridium and rhodium which it always

contains. We had to have recourse to a new process, the accuracy of which appears to be perfect as regards the elimination of iridium, and which succeeds for rhodium on condition of a little of the platinum being carried away with the latter.

Pure lead, obtained by calcining pure acetate of lead, is the solvent we employed to effect these separations.

Let us suppose that some thin plates of that complex alloy which is met with in commerce under the name of platinum are fused with from six to ten times their weight of lead. This latter will dissolve the copper, the palladium, a portion of the iron, and a small quantity of platinum. These will be dissolved, together with the excess of lead, by pure nitric acid. There will remain an alloy of platinum and lead, which weak aqua regia will dissolve, carrying away some rhodium. The final residue will be a crystallized alloy containing iridium, ruthenium, and iron, insoluble in lead.

If the platinum is rich in rhodium, this, when combined with the lead, will not be dissolved by concentrated aqua regia, but will be separated from the iridium mixed with iron and ruthenium by boiling concentrated sulphuric acid.

The platinum will be separated from the rhodium and lead dissolved with it in the aqua regia by sal ammoniac; but, to completely eliminate the rhodium, the chloroplatinate of ammonia (or platinum yellow) must be precipitated in powder so fine as to appear amorphous and nearly white. It is to be washed with water acidulated with hydrochloric acid, which removes a small quantity of platinum. This last method was communicated to us by M. Stas.

The platinum is fused, by means of our blowpipe, in a crucible of pure lime, or at least free from iron. When the metallic mass is well liquefied and has undergone the action of the refining-fire, we close briskly the two cocks which admit the illuminating-gas and oxygen, and thus solidify the surface of the ingot. The lime, being strongly heated, keeps the lower part in fusion; and the shrinking takes place on that side, most often so as to permit the cavities which form there to communicate with the exterior. By working thus we have obtained the highest densities, measured upon masses of 200–250 grammes of pure platinum. If we had possessed greater quantities of platinum, we should have cast large ingots and taken specimens of the metal from the centre or the perfectly sound parts of the ingots. On such specimens we intend to investigate the action of cold-beating and annealing—a subject of great interest.

2. *Iridium.*—The iridium made use of was extracted from materials belonging to M. Matthey, and prepared by our methods; we attacked it afresh, to clear it of the last traces of impurity. This iridium, finely pulverized, either in a mortar or by dissolving it in zinc which was afterwards driven off by heat, was fused with lead. The ingot, treated with nitric acid, aqua regia, and boiling sulphuric acid, left the iridium crystallized and still containing some ruthenium (without a trace of osmium) and a little iron. It was

attacked by its weight of nitrate of barium and four times its weight of baryta, or else by five times its weight of binoxide of barium in a crucible of silver or porcelain. The material, pounded, mixed with four or five times its weight of water, is treated with a current of chlorine in a tubulated retort with an emery stopper. When it is supersaturated with chlorine, it is distilled in a very slow current of that gas. Volatile hyperruthenic acid is obtained, which passes at first in red crystals or minute drops, which afterwards dissolve in the water proceeding from the distillation. The iridiate of barium is transformed into green perchloride of iridium and chloride of barium, with disengagement of oxygen. The liquor deprived of baryta by titrated sulphuric acid, is evaporated to dryness to separate the silica. The residue, taken up by water, consists of red-brown bichloride of iridium, the green chloride having lost some chlorine during the evaporation. We precipitate it by sal ammoniac, and wash for a long time with a semisaturated sal-ammoniac solution the deep-violet chloro-iridiate of ammonia, which we afterwards calcine in a current of hydrogen, and thus obtain metallic iridium. This, treated with nitre and potash in a gold or silver vessel, gives a violaceous mass which, taken up by water, colours it violet or deep blue. The residue is washed successively with water, dilute hydrochlorate of ammonium (which removes the potash), oxalic acid (to dissolve the iron), chlorinated water, and, finally, ammonia to take up the gold or silver*.

The iridium, strongly calcined in a crucible of charcoal purified by chlorine, is afterwards fused in pure lime with the precautions indicated for platinum ; only pure dry hydrogen must be substituted for illuminating-gas.

Densities. (1) *Pure Platinum.*—To arrive at the determination of its density, we fused our ingots again a great many times, always obtaining numbers close to 21·5 when the ingots presented the proper appearance. We here give the details of the experiment which gave the highest density :—

Weight of the platinum in air at 17°
and 764 millims. ....} 204·141 grammes.
Loss of weight in water at 17°·6 ......   9·498   ,,
Density (uncorrected)† ....   21·504

Other experiments gave numbers between the limits 21·48 and 21·50. The metal did not contain impurities in sensible quantity.
When the ingots are rolled, their density is diminished. This is

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* If a silver vessel is used, or the presence of rhodium is suspected, the reduced material must be again attacked with bisulphate of potass, and then with nitric acid and ammonia.

† To obtain D_0 the density at zero, the density D_t must be introduced in the formula

$$D_0 = \frac{(D_t + 0·001)(1 + kt)}{V_t},$$

in which k is the coefficient of dilatation of the metal, and V_t the volume of water, at the temperature of the experiment.

because the contained cavities are closed by the pressure and can no longer be filled with water.

(2) *Iridium.*—The metal, reduced to perfect liquidity, carefully cooled, and crushed in a rolling-mill, appears in the form of shining white grains with curved facets. Its density was calculated from the following elements :—

> Weight in air at 17°·5 and 760 millims... 243·292 grammes.
> Loss of weight in water at 17°·5 ...... 10·851
> Density (uncorrected) .... 22·421
> The ingot rough from casting, before } 22·239 grammes.
>   crushing, weighed .... }
> A specimen of another preparation, after } 22·403 ,,
>   crushing .... }

The platinum and iridium were analyzed : the former contained a little rhodium, and the latter some traces of ruthenium. The proportions may be calculated from the following analysis of the 10-per-cent. alloy :—

(3) *Alloy of Platinum* 90 *and Iridium* 10.—Density :

> Weight of the material in air at 18° } 238·694 grammes.
>   and 760 millims. .... }
> Loss of weight in water at 17°·5 .... 11·043 ,,
> Density (uncorrected)............. 21·615 ,,
> The contraction $\left(1-\dfrac{\Delta}{D}\right)^{*}$ is equal to 0·0012 ,,

The analysis of 8 grammes gave :—

> Platinum .... 89·91
> Iridium .... 9·93
> Rhodium........... .. 0·05
> Ruthenium .... 0·01
> Loss .... 0·10
>   ————
>   100·00

(4) *Alloy of Platinum* 85, *Iridium* 15.—Density :

> Weight in air at 17°·5 and 763·5 millims. 197·734 grammes.
> Loss of weight in water at 17°·5 ...... 9·147 ,,
> Density (uncorrected) .... 21·618
> Contraction (doubtless a trifle too little) 0·0003

The analysis of 8 grammes gave :—

> Platinum .... 85·30
> Iridium .... 14·53
> Rhodium.............. 0·05
> Ruthenium .... 0·06
> Loss .... 0·06
>   ————
>   100·00

$$ ^{*}\ \Delta = \frac{100}{\dfrac{90}{21\cdot5} + \dfrac{10}{22\cdot42}}. $$

This substance is very ductile and malleable, and its rigidity considerable. It might be very good for use.

(5) *Alloy of Platinum* 66·67 and *Iridium* 33·3.—Density:

Weight in air at 16° and 758 millims .. 53·415 grammes.
Loss of weight in water at 16° .... 2·463   „
Density (uncorrected)................ 21·874
Contraction .... 0·0034

Substance not malleable.

(6) *Alloy of Platinum and Iridium* 95.—Density:

Weight in air at 16° and 744 millims. .. 51·462 grammes.
Loss of weight in water at 13° .... 2·300   „
Density (uncorrected)................ 22·384
Contraction .... 0·0006

The material was reduced to fragments in the rolling-mill before the determination of its density.

It will be remarked that the above densities of platinum and iridium are higher than any previously found, and that the density of their alloys increases according to a very regular law—which is a presumption in favour of their purity.—*Comptes Rendus de l'Académie des Sciences,* November 15, 1875, vol. lxxxi. pp. 839–843.

EXAMPLES OF CONTEMPORARY FORMATION OF IRON-PYRITES IN THERMAL SPRINGS AND IN SEA-WATER. BY M. DAUBRÉE.

It is known that iron-pyrites, though not usually forming large masses in the crust of the earth, is extremely wide-spread, and disseminated in numerous rocks, whether stratified, eruptive, or metamorphic. Yet it is only rarely that at the present time this mineral species is detected in course of formation. To the few examples which have been certainly ascertained, I can add some which will contribute to elucidate the conditions under which iron-pyrites can be or may have been produced.

Production of pyrites in the substructions at Bourbonne-les-Bains. —The iron-pyrites has not been met with at Bourbonne-les-Bains among the various crystallized cupreous sulphurets deposited around the Roman medals, but is produced at a little distance from those incrustations, and in different parts of the subsoil.

First, in making a sounding over the exact point of emergence of the spring, there were brought up some small pebbles and grains of quartz enveloped in iron pyrites. This substance is attached to their surface, sometimes as an amorphous shining yellow coating, and sometimes in eminently crystalline crusts in which numerous triangular faces are perceptible.

What shows clearly that its formation is contemporary is, that it is attached, with the same characters, to some of the knife-shaped flints worked by the hand of man which are found at the bottom of the Roman cesspool with other antique objects. Moreover the

accumulation of quartzose sand beneath the cesspool, also brought up by the sounding-line, was subjected to a washing so as to separate from it the heaviest portions, when innumerable grains of pyrites were distinguished bristling with crystals, and less than a quarter of a millimetre in diameter. Some of these grains have iregular shapes, and appear to be the débris of coatings unlike those which have just been described ; others are rounded, like miniatures of the nodules of pyrites met with in various beds.

Further, on attentively inspecting the bricks of a Roman floor of a conduit of mineral water, I recognized pyrites there also. This mineral has been produced in the midst of the lime which envelops each brick, in small cavities, where it appears in globules of a brassy yellow, terminated by crystalline faces ; but these faces are so minute that one could not be sure that they belonged to cubic pyrites and not to marcasite : however, their fine yellow colour and their unchangeability render the first supposition the more probable.

Pyrites of contemporary formation exists in general in amorphous coatings ; this at Bourbonne is distinguished by its crystalline state.

Formation of pyrites in calcareous pisolites at Hamman-Meskoutine, Province of Constantine.—The thermal springs of Hamman-Meskoutine, renowned for their high temperature (95° C.), their copiousness, and for the development and singular form of the incrustations which they continue to build up daily in the shape of cones, have also generated pisolites comparable to the granules of Carlsbad or Tivoli.

Some of these pisolites are enveloped in pyrites. Nor is this deposit merely superficial : when a number of the globules are broken, in some of them pellicles of pyrites are perceived among the whitish very thin concentric layers of which they are composed. The centre is ordinarily a fragment of crystalline and lamellar limestone which has served as a nucleus for the concretions.

From information for which I am indebted to the kindness of M. Tissot, Mining Engineer, it appears that these pyritous pisolites, which are rare, seem to be formed in the ascending channels of the thermal springs ; they are therefore brought to light by the force of the ascending water, nearly in the same manner as the pyritous grains of Bourbonne-les-Bains. Although formed at an elevated temperature, the carbonate of lime which accompanies them is in the state of calcite, and not of aragonite ; for it does not decrepitate before the blowpipe.

Pyrites formed in wood immersed in sea-water.—The third instance of present formation of pyrites which I have to describe does not belong to the action of thermal springs, but to that of sea-water mingled with fresh water.

This pyrites was met with recently in England, in the interior of a piece of wood from the royal yacht ' Osborne.' It forms, in a fissure of the wood, a thin coating of a fine yellow colour and bright

metallic lustre. For the specimen which I present to the Academy I am indebted to the kindness of Professor John Percy, of the School of Mines, London, well known by his important works on Metallurgy. Mr. Weston, Chemist to the Admiralty, has been so good as to furnish me with information on the fact, which he was the first to observe. The ship ' Osborne ' was built at Pembroke Dockyard, and sent to Portsmouth to be finished. It was there deemed necessary to increase her thickness ; and it was in preparing for that purpose a piece of wood situated near the keel, that they discovered a cavity lined with pyrites. Before being used, this wood had lain, according to custom, for some time in a pit at Pembroke, or perhaps at Portsmouth ; at both places these basins lie between high- and low-water mark and receive a mixture of fresh and sea-water. It must be added that two sewers run into the basin at Portsmouth ; and probably the case is similar at Pembroke ; so that it is not impossible that, besides reducing and sulphuretted substances, the sewers introduce into certain parts of the basins, at least accidentally, a higher temperature than the normal temperature of the sea.

The surface on which the pyrites is deposited is much blackened, so as to resemble an ulmic substance ; and this bears witness to the reducing effect operated on the vegetable matter.

There is one mode of present formation of pyrites which is well deserving of attention, but is usually passed over in silence in geological works ; it is that described by M. Bunsen in his important memoir on Iceland[*]. As shown by that eminent physicist, iron-pyrites is produced in several localities of that country by the vapours of fumaroles charged with sulphuretted hydrogen ; these gaseous substances react upon the iron contained in the silicated rocks through which they infiltrate and which they attack, particularly palagonite and pyroxenic rocks. In this case the pyrites has crystallized very neatly in innumerable minute cubes. It is associated with sulphate and carbonate of lime, of which the base has been taken from the silicated rock and sometimes combined with sulphur in excess.

Although formed in the wet way, the pyrites of the Roman floor somewhat resembles that engendered by the Iceland fumaroles as regards its mode of dissemination in the rock in which it was produced.

By the side of these instances of the present formation of pyrites in nature, we must not forget that it was, long since, produced artificially by M. Becquerel, and afterwards by De Senarmont, with the mineral characters of the metalliferous veins.—*Comptes Rendus de l'Acad. des Sciences*, vol. lxxxi. pp. 854–859.

[*] Pogg. *Ann.* vol. lxxxiii. pp. 197–272, 1851.

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END OF THE FIFTIETH VOLUME.

ALERE FLAMMAM.

Polar
Hexagon

Fig. 2.

lat 63° A
lat 54°
B
O a b c
lat 45°
C
lat 36° D

1.3233 R

.340 R

.491 R

The radii of the circles of lat. given by R cot l.
The distances OA, OB, OC, OD by R cot (l + 37° 23')
The distances Oa, Ob, Oc by R sin 37° 23' tan γ
where γ = 9', 18', 27'
R is radius of inscribed sphere & l latitude.

Fig. 1.

Diameter of inscribed
sphere is 3 inches.

Fig. 3.

N M L Equatorial
Hexagon

lat 27°
A
B lat 18°
.5R
O lat 9°
C
D Equator

.5R

R Q P

The radii of the circles of lat. given by R cot l.
The distances OA, OB, OC, OD by R cot (l + 79° 11')
The intercepts LM, LN, PQ, PR given by R tan γ
(sin 79° 11' ± ½ cos 79° 11') where γ = 9' & 18'.

Fig. 4.

L M N

Equatorial
Pentagon

lat 45° A
lat 36° .5R B
lat 27° C
O
lat 18° D
lat 9° E

.491 R

.5R

P Q R

The radii of the circles of lat. given by R cot l.
The distances OA, OB, OC, OD, OE by R cot (l + 63° 26')
The distances LM, LN, PQ, PR, by R (sin 63° 26' ±
½ cos 63° 26') tan γ where γ = 9' & 18'.

Fig. 6.

A

F E
.6180 R

B D C

.5468 R

R is the radius of the
circumscribing sphere.

Fig. 7.

F E

A

D

Fig. 5.

A

F
E

B D C

Fig. 5.

Fig. 4.

Fig. 1.

Fig. 4.

Fig 3.

Fig. 5.

Fig 2

Air

No 1f | K Cl

No 2f | Rb Cl

No 2e | Rb Cl

No 3f | Cs Cl

No 3e | Cs Cl

No 4f | Tl Cl

No 4e | Tl Cl

No 5f | Na Cl

Sunlight

A   B   C   D   E b   F   G   H H

Air

№ 5e Na Cl

№ 6f Li Cl

№ 6e Li Cl

№ 7f CaCl$_2$

№ 7e CaCl$_2$

№ 8f Sr Cl$_2$

№ 8e Sr Cl$_2$

№ 9f BaCl$_2$

№ 9e BaCl$_2$

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

130   140   150   160   170

THE

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